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THE RESISTANCE OF SHIPS

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NOTE.

Professional Paper No. 23 is a reprint of papers "On the Resistance of Ships," by the late W. Froude, esq., M. A., F. R. S., Vice-President of the Institution of Naval Architects, together with a reprint of a paper by R. E. Froude, esq., Associate Member of the Institution of Naval Architects, "On the leading Phenomena of the Wave-Making Resistance of Ships."

CONTENTS.

1.

EXPERIMENTS FOR THE DETERMINATION OF THE RESISTANCE OF A FULL-SIZED SHIP, AT VARIOUS SPEEDS, BY TRIALS WITH H. M. S. "GREYHOUND." PERFORMED OFF PORTSMOUTH IN AUGUST AND SEPTEMBER, 1871.

These experiments were instituted by the Lords Commissioners of the Admiralty, at the request of the committee on designs for ships of war, on whose attention the importance of the object had been strongly pressed by Mr. G. P. Bidder, a member of the committee.

Mr. Bidder and myself (also a member) were appointed a subcommittee to carry out the experiments; but as Mr. Bidder's other engagements did not admit of his devoting to the investigation the uninterrupted attention, extending over a considerable period,* which it would manifestly require, it was decided, after we had carefully discussed together and agreed on the design of the apparatus to be used and the *modus operandi* to be pursued, that the conduct of the investigation should be left in my hands.

I had the advantage, however, of Mr. Bidder's presence and counsel during many of the experiments, and their progress was occasionally watched by other members of the committee.

The point to be determined was the resistance experienced by a ship of considerable size and of known form and dimensions, when moved through smooth water at various speeds.

This point had never been determined with even approximate exactness; such information as has been possessed relevant to it had been derived almost solely from measured-mile and other similar trials made with steam-ships. But in all such trials the friction, the air-pump duty, and other resistances of the engine, and the resistances involved in the action of the propeller, are inextricably interwoven with the deduced resistance of the ship. The result obtained is not the simple result sought, and indeed, as will be seen, differs widely from it.

Hence the results of this inquiry are of fundamental importance, if only from the light which, when compared with those derived from steam-ship trials, they tend to throw on the subject of engine friction and of the action of propellers.

Moreover, they have special value as affording data for testing, or possibly correcting, the formula by which Professor Rankine has endeavored to express approximately the probable resistance of a ship of given form, or, again, for assigning numerical values to the constants embodied in any formulæ which may be suggested by other investigators.

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* The experiments occupied fully six weeks, including the few days when they were interrupted by unsuitable weather.

They have also an important bearing on another experimental inquiry, which is being carried on by me under the sanction of the Admiralty, namely, the endeavor to determine the resistances of a ship of any given form by the much simpler process of determining those of a sufficiently large model of the ship—a method the value of which depends on the correctness of the scale of comparison by which the resistances of the ship are inferred from those of the model. The scale which has been propounded possesses undoubted prima facie theoretical truth and some experimental justification, and would be tested completely and might receive correction by help of the trial of a full-sized ship.

In the conduct of the experiments more difficulties were to be mastered than are at first sight obvious.

To obtain a tolerably satisfactory determination of a ship's speed in the usual method on the measured mile requires, for each speed, a succession of many mile runs alternately with and against tide; and from the variation which may meanwhile be occurring in the speed of the tide some elements of doubt are even then involved in the final average.

To complete a measured-mile trial satisfactorily with a ship under her own steam involves in the turnings considerable care, and is at best a lengthy operation. When one large ship is towing another large ship, the difficulty and the expenditure of time would be increased enormously; and, bearing in mind the number of speeds to be tried and the variations of trim and immersion which the experiments, to be complete, should contemplate, and, lastly, the uncertainties of weather, the process would be interminable and extremely costly. Again, the variations of towing force, which must inevitably occur during such a lengthened trial, would render the record of mere dynamometric strains uncertain and delusive.

It was in fact obvious that, to be satisfactory and conclusive, the records alike of speed and of force must be continuous and automatic; and if such a speed record could be obtained, it would be comparatively easy to combine with it a force record, by automatic arrangements of well-known type; but to obtain such a speed record was a problem which had not yet been even approximately solved

The method adopted was one suggested by Mr. Bidder, which prima facie promised complete success. It consisted in paying out a continuous length of twine attached to a log-chip of large area, the twine as it ran passing round and gripping a counting-wheel of definite circumference, which was geared so as to transmit the motion thus obtained to a revolving cylinder charged with a long sheet of paper, the length of which occupied the entire circumference of the cylinder. The circumferential travel of the sheet thus represented the ship's travel on a reduced but measurable scale; and on it the force indications of the dynamometer and a time-scale supplied by a piece of clock-work were simultaneously and automatically marked.

The sheet of paper when removed from the cylinder exhibited in fact a straight base line traced along one of its edges by the time-pens, carrying a series of slight indents, the intervals between which corresponded with the lapse of two and one-half seconds, the lineal measure of each interval being one 1000th part of the space traveled by the ship during the time interval; while a separate pen, the distance of which from the base line was governed by the dynamometer, and thus indicated the towing strain, continued to record the momentary amount of the force opposite the corresponding moment on the time-scale.

By carefully scaling the time intervals throughout the course of each experiment, it was easy to see approximately whether the speed was steady and what was its amount.*

The several parts of the apparatus thus sketched, and the mode of their application, deserve perhaps a fuller description. The principle of construction adopted for the dynamometer may be described as the converse of that of the hydraulic press. The direct strain of the tow-rope was received by a piston 14 inches in diameter, pressing on oil, inclosed in its cylinder. The oil, under the pressure thus applied, had access to a second cylinder, the piston of which, 1⅛ inch in diameter, was held in check by a spiral spring; and the extensions exhibited by this spring, under the pressure supplied by the oil, were proportional to, and formed a measure of, the direct strain of the tow-rope. These extensions, reproduced on an enlarged scale by a properly proportioned index arm, were traced automatically on the paper which covered the revolving cylinder.

It should be observed that the true towing-strain is less than the actual tension of the tow-rope, because this last is a resultant which includes a proportion of the dead weight of the rope. It is the horizontal component alone of this tension, or the element which operates in the line of motion of the ship, which it is proper to record.

This separation was effected by bringing the drag of the tow-rope with a clear lead to a stout framework on wheels, or truck, which rested on a short piece of railway, planted level and fore and aft on the extreme forward end of the topgallant-forecastle of the ship which was being towed. The after end of the truck was linked to the dynamometer; the dynamometer itself rested on the aftermost end of the longitudinals which carried the rails, not bolted to them, but simply anchored in position (so to speak) by powerful fastenings carried horizontally sternward and secured to the bitts. The instrument therefore formed a dynamometric link in a chain, strained simply by the horizontal component of the tow-rope tension.

With cylinders intended for the communication of hydraulic pressure, it is usual to secure the necessary tightness of fit in the piston and

* Bearing in mind that a speed of 1 knot very nearly corresponds with 100 feet per minute, it is at once seen that the speed of the ship in knots might be read off by measuring the length of a single interval on a scale of 20 to the inch, or, more conveniently, the length of four intervals on a scale of 5 to the inch.

piston-rods by either elastic packing or cup-leather arrangements. But all such methods are known to involve considerable friction, which must unavoidably detract from the exactness of the determination of the fluid pressures experienced by the piston; and it appeared possible to secure sufficient immunity from leakage coupled with freedom of motion, by goodness of workmanship and exactness of fit, in the first place, and, in the second, by adopting the method of circumferentially grooving one or other of the fitted sliding surfaces.*

The dynamometer was constructed to carry any strain up to 20 tons; and this would be quite sufficient for the performance of its contemplated proper duty. But since in getting up speed, and in turning, the tow-rope would probably deliver strains sometimes excessive and sometimes oblique, for which it seemed undesirable to provide in the construction of the instrument itself (because the provision would have complicated its construction and increased the weight of its working parts), it was necessary to supply an independent appliance, which could on an emergency be made to relieve the dynamometer of all strain, and which would at all times guard it against lateral strains of undue magnitude.

This was accomplished by erecting a pair of strong hardwood shears, from the cutwater of the ship, at a short distance in front of the dynamometer-truck. The feet of these were upheld by a strong bolt which passed through the cutwater, and on which, acting as a pin-joint, they were capable of rocking in a fore-and-aft plane; their heads were bolted together with a blocking-piece between them of a thickness equal to that of the cutwater, so that between them a parallel vertical space of that width was kept open, through which the tow-rope passed to the truck. The fore-and-aft motion of the head of this rocking frame was governed by a system of purchases, bv which it could either be pushed forward or held back; and against lateral strain it was fortified on each side by strong chain rigging, secured below to the stem, and to which sufficient spread was given by lateral strutting from the upper part of the cutwater.

The tow-rope, leading clear through the opening in the rocking frame,

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* The skill with which this difficult part of the work was executed by Messrs. Kittoe and Brotherhood, the makers of the dynamometer, deserves special notice. The final fitting of the 14-inch piston in its cylinder, and of the piston-rod in its gland, was completed by the scraper; the fit was at once so close that the leakage under the heaviest pressures was merely nominal, and so uniform that frictional tightness was completely obliterated, and, when the cylinder was empty, a force of about 30 pounds was sufficient to move the piston throughout the length of the cylinder. It is true that when the cylinders were filled with oil a semiviscous adhesion between them and the pistons, somewhat indeterminate in its amount, made its appearance; but this was incomparably less serious than would have arisen from any kind of elastic packing applied to the piston and piston-rod. The instrument was tested to 22 tons strain at Mr. Kirkaldy's Testing Works, and the calculated scales appropriate to the several springs verified.

was secured to a strong hardwood bar or "toggle;" and this in turn was secured by a chain to the dynamometer-truck.

When it was desired to relieve the dynamometer of all strain, the head of the rocking shears was hove sternward, pulling with a leverage of about three to one on the toggle, and slacking the chain between the toggle and the dynamometer-truck; and this relief was always given when any sudden increase of speed was contemplated, or any severe lateral strain was to be called into play in turning.

The dynamometer being planted on the topgallant forecastle, the chain by which it was anchored to the bitts was led over a bolster planted on the break of the forecastle, at such a level that the drag or reaction of the chain, the axis of the cylinder and piston-rod, and the drag-link by which the latter was secured to the truck should be as nearly as possible in one straight line—the object of this precaution being to obviate the friction which would have been induced by oblique strains. And the precaution was on the whole successful, though while the experiments were in progress the total frictional resistance, instead of being limited to the 30 or 40 pounds offered by the piston and piston-rod with the cylinder empty, was found, when inclusive of the viscous adhesion of the oil already noted, and under maximum towing-strains of 25,000 or of 30,000 pounds, to amount to 150 or 200 pounds.

It proved easy, however, to eliminate the errors which this circumstance tended to introduce into the record, by administering a succession of smart blows with a mallet to the cylinder or the framework on which it rested; for the tremor thus occasioned served to release the moving parts from frictional adhesion.

It was, of course, essential to the accuracy of the experiments that the ship which was being towed should not be immediately in the "wake" of the towing-ship, but should, on the contrary, be in water that was, as far as possible, undisturbed. To attain this condition, even approximately, by the use of an exceedingly long tow-rope, would have been impossible in the narrow waters inside the Isle of Wight, to which, for the sake of smooth water, it was generally necessary to confine the experiments. Moreover, the alternate tightening and slackening always observable in a long tow-rope would render the diagram of resistance so irregular as to require experiments of considerable duration, in order to get a really accurate result. The only alternative was to keep the towed ship on one side of the wake of the towing ship; and this was done by rigging out a 45-feet boom from the starboard side of the towing ship amidships, and leading the towing hawser from her bow through a block at the end of this. The length of hawser used was such as to bring the bow of the towed ship about 190 feet clear of the stern of the towing ship. This was considered far enough astern to avoid any implication of the towed ship in the "stream-line" motions of the water surrounding the towing ship, while to tow her with greater scope would most probably have brought her within the widening range of influence of the "wake." It was also necessary that the log should be hove clear of the wake of both the towing and the towed ship; and with this object the log-line was led through a sheave in the end of a 20-feet spar, rigged out from the starboard side of the towed ship, and the log-chip was dropped into the water immediately under this. The log-chips were about 2½ square feet in area, and were ballasted and buoyed so as to sink about 4 feet under water, and present themselves square to the line of motion. Some difficulty was found in paying out the log-line, owing to the tendency of the reel of line to occasionally overrun the demand of the log, and then, becoming in turn retarded by its friction, to snap the line when the slack was taken up. To prevent this, it was necessary to put a brake on the reel, entailing a strain (regulated pretty uniformly to about 2 pounds) on the log-line. The log-line, consisting of twine of good quality, was saturated with tallow; this improved its buoyancy, and effectually prevented it from contracting when immersed.

An observer, stationed on the poop of the towing ship, noted the number of revolutions per minute of her screw-propeller, and communicated each observation, as soon as it was made, to the towed ship, by writing on a blackboard. Subsequently, when this count was found to supply an effective check on the indications of the log, an observer noted the counter in the engine-room every minute, connecting his record, by comparison of watches, with that kept on board the towed ship. Each diagram taken was also carefully connected with the latter by making a mark on the paper opposite the diagram-pens at a certain instant of time noted in the record.

The speed of the wind past the ship was noted by a wind-gauge erected on a flagstaff on the forecastle of the towed ship. In the later experiments the indications of the wind-gauge were automatically recorded on the dynamometric cylinders by an electrical arrangement.

In interpreting the naked record of simultaneous speed and resistance, exhibited by the dynamometer, it was necessary to apply certain corrections, as follows:

(1) It was necessary to allow for the action of wind on the hull of the towed ship. It has been mentioned that the speed of the wind past the ship was carefully recorded by the wind-gauge throughout each experiment; and the force due to this ascertained speed was estimated by the following experiment: On a favorable occasion the ship was allowed to drift before the wind, and the speed she thus attained through the water was registered by the automatic log. Since the resistance offered by the water at that speed was already known by the dynamometric experiments, it formed a measure of the effective force of the wind which was then blowing, and the speed of which, relatively to the ship, was indicated by the wind-gauge. The speed of the wind past the ship was 15 knots, and its force, calculated in the manner described, was 330

pounds; and in estimating, from this result, the wind correction applicable to any given towing strain, it was assumed that the propulsive or retarding force of the wind on the ship varied as the square of the mean speed of the wind past the ship in the line of her motion during the experiment.

(2) Wherever the speed of the ship was not quite uniform throughout an experiment, it was necessary to regard the registered strain on the tow-rope as representing not simply the resistance of the water at the speed at which she was at the instant moving, but that resistance *plus* or *minus* the force due to the acceleration or retardation of the mass of the ship and the water surrounding her; and this force it was, of course, necessary to ascertain and eliminate from the recorded resistance. The speed-diagram on the recording cylinder indicated the existence and the amount of any inequalities in the speed; and the force which must have been employed in producing them would have been at once ascertainable by calculation, if the momentum of the ship alone had been all that had to be taken into account. But according to the "stream-line" theory, which is now almost universally accepted, the motion of a ship through the water is accompanied by an extended system of motions in the surrounding fluid; and any acceleration or retardation of the ship involves a simultaneous alteration of the respective velocities of every part of this system, the force necessary to cause these being derived from the ship. In fact, the effect of these motions in the surrounding water is tantamount to an increase in the dead weight of the ship. A measure of the amount of this increase was necessary, in order to make the errors due to acceleration calculable; and this was obtained by the experiment of "slipping" the tow-rope when at high speed, and observing by the automatic record the rate at which the speed of the ship was destroyed by her resistance. The resistance being already approximately known, the momentum involved was, of course, at once determinable.

(3) The tension of the log-line, due to the brake by which it was necessary to restrain the log-reel, was sufficient to cause a very sensible "slip" or travel of the log-chip (large as its area was) through the water. The slip, however, was probably of fairly uniform amount, as the tension was kept tolerably constant, and the log-chips used were always of the same dimensions. The amount of this "slip" was ascertained by experiment, and was about three-tenths of a knot.

(4) It was necessarily the case that during the progress of an experiment the ship would run some distance from the log-chip, and possibly into a tide having a speed to some extent different from that in which the log-chip was situated. In such a case the indicated speed would clearly become erroneous to the extent of the difference of speed of the two tides; and this I think to have been by far the most serious element of error in the experiments. However, their accuracy was not dependent on the indications of the log alone, since a collateral measure

of speed was afforded by the revolutions of the screw-propeller of the towing-ship. This is a more reliable measure of speed than might be at first supposed. It is true that the necessary slip of a screw is a rather large percentage of the speed; but under the circumstances of these experiments there was probably no cause for variation in the amount of slip, except the known variations in the amount of the resistance to be overcome. Thus, for the *relative* speeds of different experiments, the revolutions of the screw formed a measure of speed perhaps more reliable than the log used, considering the liability of the latter to error owing to the cause already mentioned. But the rate of revolution of the propelling screw could not be used as a measure of *absolute* speed without data for obtaining a correct co-efficient for interpreting "revolutions per minute" into "feet per minute;" and these data were reliably supplied by the logged speeds, which formed, when duly grouped, large bodies of evidence, the average results of which could be confidently accepted. Of course, the values of these averages were, to some extent, different under the different conditions of experiment, according as these conditions entailed greater or less resistance of the towed ship; but such differences could be allowed for with accuracy when the total extra slip of propeller screw, due to the total resistance of the towed ship, had been fairly determined.

The experiments were carefully analyzed, with a view to obtaining all these particulars; and in cases where, after applying all other corrections, the results of individual experiments were still discordant, it was found that the discord was generally remedied by thus correcting the speeds by the revolutions of the screw propeller. Accordingly the final results given have had this correction universally applied.

The towing-ship was H. M. S. *Active;* the ship towed was H. M. S. *Greyhound*, screw sloop, 878 nominal tonnage, without masts, brought by ballast to the required displacements and trims. She was tried at three different displacements, and at each with different trims. Ultimately, bilge-keels 3 feet 6 inches wide and 100 feet long were affixed, one on each bilge, and thus fitted she was tried again at three different trims. In each condition she was tried at various speeds, ranging from about 3 knots to 12½ knots. The following are the particulars of the different conditions of trial:

Description of displacement.	Mean draught.		Area of midship section.	Displacement.	Immersed skin.
	Ft.	*In.*	*Sq. ft.*	*Tons.*	*Sq. ft.*
Normal displacement	13	9	339	1,161	7,540
Medium displacement	12	11½	313	1,050	7,260
Light displacement	12	1	284	938	6,940

Description of trim.	Reference number.	Draught.		Difference.	
		Forward.	Aft.	By the head.	By the stern.
		Ft. In.	*Ft. In.*	*Ft. In.*	*Ft. In.*
Normal displacement:					
By the head	1	14 6	13 0	1 6	....
Normal trim	2	13 6	14 0	....	0 6
By the stern	3	12 6	15 0	....	2 6
Greatly by the stern	4	11 6	16 0	....	4 6
Medium displacement:					
Greatly by the stern	5	10 8½	15 2½	....	4 6
Light displacement:					
By the head	6	12 8	11 6	1 2	....
Normal trim	7	11 10	12 4	....	0 6
Greatly by the stern	8	9 10	14 4	....	4 6
With bilge-keels:					
Normal displacement:—					
By the head	9	14 6	13 0	1 6	....
By the stern	10	12 6	15 0	....	2 6
Greatly by the stern	11	11 6	16 0	....	4 6

A sheer plan and a half-breadth plan of the hull of the ship are shown in figures 1 and 2 (Plate I). Figs. 3, 4, and 5 (Plates II and III) show the load water-line at each of the above draughts, distinguished by the reference numbers.

The results of the trials are graphically exhibited in the accompanying diagrams or curved lines, Figs. 6, 7, 8, 9, 10, 11, 12 (Plates IV and V), in which the horizontal measurements or abscissæ represent speeds, and the vertical measurements or ordinates represent the corresponding resistances. The diagrams thus constructed may be termed "curves of resistances."

Fig. 6 (Plate IV) shows the resistances under condition No. 2 for any speed within the range of trial. The small black spots on the diagram indicate the results of the individual experiments after making the necessary corrections, and form the authority for the curve. Fig. 10 (Plate V) shows in a similar manner the resistances under condition No. 5.

Figs. 7, 8, and 9 (Plate IV) represent, respectively, the resistances under several various conditions, shown together for the purpose of comparison.

In proceeding to compare the results shown by the several curves of resistance, it must be observed that the minutiæ of the features they present can not be insisted on as absolutely exact, because minor discrepancies in result were in some cases noticeable when an experiment was repeated with unchanged conditions. The reliance to be placed on them, however, as substantially correct, will be reinforced by tracing the general correspondence between the changes of resistance which were induced by the several changes of trim when repeated with normal displacement, with light displacement, and with the addition of bilge-keels.

A comparison of the several curves leads to the following conclusions as to the character of the law of resistance of this particular ship under the several conditions.

As dependent on differences of speed it appears that, up to about 8 knots, the resistance is almost exactly proportioned to the square of the speed. With normal displacement and trim it is thus very exactly expressed in pounds by the term 88 V^2, being about 5,600 pounds at 8 knots. Above 8 knots it increases more rapidly, so that at 12.8 knots, the highest speed attained, instead of being only 14,400 pounds, as it would have been if the law had been unchanged, it has gradually risen to 24,000 pounds.

As dependent on differences of trim, the resistance does not change largely; indeed, at speeds between 8 and 10 knots it scarcely changes appreciably even under the maximum differences of trim. In proportion as the ship is down by the head the resistance is, on the whole, increased at the higher speeds and diminished at the lower, and this character of difference is maintained at both "normal" and "light" displacements. Comparing the resistances under the extreme conditions of the ship ("by the head" and "greatly by the stern"), the difference is from 7 to 8 per cent. at 12 knots, and perhaps 10 per cent. at 4 knots. With the bilge-keels added, however, the advantage of the ship at high speeds when trimmed "greatly by the stern" is not sustained.

As dependent on differences of immersion, the resistance is decidedly less at "light" than at normal displacement; but the difference is certainly not proportionate to the difference in area of midship section, as it might perhaps have been expected to be. Fig. 11 (Plate V) shows a curve representing the average result of conditions Nos. 1, 2, and 4, normal displacement, compared with one giving the average result of the similar conditions at the light displacement, viz, Nos. 6, 7, 8, and from these it will be seen that the average resistance with the light draught between the speeds of 800 and 1,200 feet per minute is about 10½ per cent. less than with the deep draught, there being an accompanying reduction of about 16¼ per cent. in area of midship section, of about 8 per cent. in area of wetted surface; and, what is far more material from an economical point of view, of 19¼ per cent. in displacement, and assumably, therefore, in possible engine-power; it seems probable, too, that at a still deeper draught than with the normal displacement, the resistance would not be increased proportionately to the displacement.

The excess of resistance indicated as due to the bilge-keels is considerably less than should be caused by their surface-friction alone. Calculating from the data afforded by the experiments on surface-friction made by me for the Admiralty, and making full allowance for the fact that portions of the surface of the bilge-keels would be implicated in the belt of water set in motion by the skin of the ship, the addition of these keels should cause an additional skin-resistance of 800 pounds at a speed of 10 knots, and varying approximately as the square of the speed, whereas the experiments with the ship imply that the additional resistance was only about 350 pounds, taking an average of the results obtained at speeds of from 8 to 12 knots, as may be seen from Fig. 12

(Plate V). I think it most probable that this discrepancy is due to some difference in the condition of the surface of the ship on the separate occasions of experiment, which may have arisen from her remaining in the dock for a week (when, however, her copper was not cleaned) while the bilge-keels were being fitted.

The experiment of "slipping" the towed ship when at high speed was not only desired for the purpose of correcting the indications of the individual experiments for the effect of irregularities in speed, but was also very valuable as forming an important test of that proposition in the theory of "stream-line" motion, as applied to ships, which was explained above.

Four experiments of this type were made with the ship at normal displacement, two with her at light displacement; the former under the conditions of trim numbered 2, 3, 4, and 11, the latter under those numbered 6 and 8.

The results of these experiments are given in Figs. 13 and 14, respectively (Plate VI), as reduced to the final abstract form of "curves of retardation."

In these curves the abscissæ express speed of ship in feet per minute. The ordinates express the "rate of retardation" experienced at these speeds in terms of the loss of speed that would ensue in an interval of 100 seconds, supposing the rate of retardation to continue constant for that period. The intervals of time into which the course of each experiment was subdivided in determining the progressive retardation were in fact only $1\frac{1}{4}$ seconds; but in expressing the measure of the rate, the larger figures are more convenient.

To arrive at the results, the first step was to lay off the data obtained in each experiment as a curve, in which the abscissæ expressed "time" and the ordinates "travel."

Graphic differentiation of the curves thus constructed gave of course a corresponding series of diminishing speeds; and these were laid off as curves in which the abscissæ expressed "time" and the ordinates "speed."

The speed-curves were in turn graphically differentiated, and gave the retardations experienced at the several speeds; and the retardations thus determined are used in constructing the final or abstract curves given in Figs. 13 and 14 (Plate VI).

A companion curve, given in dotted lines in each figure, expresses, in the same terms, the rate of retardation which would have been exhibited if it had been the ship's mass taken alone and uninfluenced by the companion motions of the surrounding water, that was being retarded by her known resistance at the several speeds.

The irregularities of the curves obtained by the differentiation require explanation; they result, no doubt, partly from the circumstance familiar to all who are engaged in investigations of this kind, that in graphic solutions, unlike those of pure mathematics, "Differentiation" is more difficult than "Integration."

This inevitably follows from the smallness of the quantities to be taken account of, since, within certain limits, the absolute magnitude of the error we make in measuring a small quantity is likely to be as great as that we make in measuring a large one; so that its relative magnitude will be great, in proportion as the quantity itself is small.

Thus, whereas if the ship's speed were steady we might count it by the distance she traveled in several consecutive minutes, we must count it on the contrary, while she is undergoing retardation, by the distance she travels in but as many seconds. Hence an error of given magnitude, in time or space, will in the latter case be in effect sixty times as great as in the former.

But besides these errors, which perhaps should be rather termed inexactnesses, it can not be doubted that tidal irregularities (such as have been already adverted to) must have introduced into the results sensible errors, which were here of the more importance since, from the nature of this retardation experiment, the speed-correction which the revolutions of the *Active's* screw furnished during the towing experiments was no longer available.

It was possible in fact that the slip, small as it was, of the log-chip would occasionally carry it from a slower into a more rapid current, or *vice versa*—circumstances under which the consequent speed-errors would be at a maximum, since the whole difference of tidal speed would thus be introduced into the speed-record, without any real change in the speed of the ship.

Or again, as must have happened more frequently, the ship herself might traverse water having varied tidal speed, and would therefore at once experience the altered rate of retardation due to this adventitious alteration in her speed through the water, of which the speed-record could take no cognizance.

It is of course impossible to say to what exact extent either of these forms of error was in reality the cause of the greatest of the irregularities instanced; but it is possible, and at the same time instructive, to trace out to what extent one or the other must have been called into existence if the whole of the irregularity apparent in the worst of these curves, namely, that under condition No. 4, were attributable to it. The result is as follows:

The experiment in question occupied a period of not quite seven minutes, which we may divide into three approximately equal parts, corresponding with the main features of the irregularities which its results contain. And the irregularities are such as would have arisen if, during the successive intervals, the log-chip had deviated into tidal changes such as to carry it first ahead at a mean speed of 15 feet per minute, then astern at the same speed, and then again ahead at 18 feet per minute. Or, again, they would have arisen if, during the successive intervals, the ship had encountered first a relatively adverse tide of 75 feet per minute, then a relatively favorable one of 120 feet per minute, and lastly an adverse one of 180 or 190 feet per minute.* With this explanation I think that even the worst of the irregularities which the curves of retardation present can not be held to invalidate the conclusion to which their general character points. This may be best arrived at by a comparison of the mean of the areas which the several curves of retardation contain on a given length of base, with that of the dotted curve which shows what would be the rate of the ship's retardation due to her resistance if her own mass alone were the basis of the calculation. The ratio of the former area to the latter will be the inverse ratio of the ship's *virtual* mass (including the dynamic effect of the surrounding water) to her actual mass. The smaller the rate of retardation the greater must be the virtual mass.

Treating thus the curves in Figs. 13 and 14, the former of which relates to the ship at her normal displacement, the latter at her light displacement, her virtual weight appears to be in the former case 1.20, in the latter 1.16 her actual weight. It is not improbable that the ratio would be somewhat the smaller with the ship at the lighter draught; at all events as only one experiment was tried under the latter condition, while under the former condition there were five, it seems that the true ratio can not well be more than, though probably is not much under, 1.20.

With the object of testing the results of the retardation experiments by examining the operation of the same principle from a somewhat different point of view, two experiments were made in which the ship was subject to the greatest accelerating force which the towing power of the *Active* and the strength of the tow-rope permitted.

It had, indeed, appeared at first sight that a far more critical test of the principle could thus be applied; for it seems as if the very greatest strain which the tow-rope could bear might be applied to the *Greyhound* when actually or nearly motionless, and when therefore the effect of her resistance and the possibility of error in estimating it would be almost eliminated.

But, unfortunately, this hope could not be realized, because, in order to commence thus from a state of rest, it appeared on reflection that it would be necessary to have the tow-rope already horizontal, yet without strain; for, with it hanging in a bight, it was impossible for the *Active* to go ahead full speed, since the sudden strain brought to bear by its being straightened would have been altogether inadmissible; yet so small was the ship's resistance at low speed, that the tow-rope was not approximately straightened until a speed of about 6 knots was at-

* Neither of these suppositions is in fact a forced one. Equal or even greater irregularities would have been frequently encountered but for the skill with which the pilot in charge selected the courses to be traversed; indeed, in spite of all precaution an experiment in progress had to be occasionally interrupted when it was seen that the ship had crossed a tidal line; and this may have happened in other cases when, owing to wind or other causes, the surface-ripple by which such transitions are generally distinguishable was obliterated.

tained, nor could this condition of an effective start have been established except by securing the stern of the *Greyhound* to some ship at anchor, by a slip-rope which should be let go only when the strain, after having been gradually brought to bear, had risen to a sufficient maximum.

The numerous practical difficulties which this arrangement would have involved prevented it from being seriously contemplated; and the results of the two experiments tried are in consequence less instructive than those of the retardation series. They are represented in Figs. 15 and 16 (Plate VI) in terms analogous to those used in Figs. 13 and 14, having also been deduced from the speed-diagrams by an analogous method, simply substituting acceleration for retardation, and taking as the operative force at each instant not the resistance due to the speed, but the recorded tow-rope strain minus that resistance.

Here, also, in each figure, a dotted line shows what would have been the rate of acceleration had the accelerative force acted on the ship's mass alone; and a comparison in each case between the area of this curve and that of the corresponding curve of acceleration, deduced from the diagrams, measures the ratio of the ship's virtual and actual mass.

The ratio, as thus deduced from Fig. 15, is 1.075, and as deduced from Fig. 16 is 1.065, being in both cases considerably less than that obtained from the retardation experiments.

The very great alternations of force which were involved in the alternate tightening and slackening of the tow-rope, which it seemed impossible to avoid while the acceleration was in progress, and the effects of which are manifest in the great and characteristic irregularities of the curves of acceleration, detract considerably from the reliance to be placed on those results; and on the whole I am inclined to think that the ratio 1.20 which was deduced from the retardation experiments is to be accepted in preference.

In reference to the question of waste through engine-friction, and defective efficiency of propeller, I subjoin a comparison between the results of these experiments and the performance of the ship on the measured mile. I have selected for this purpose a trial of the *Greyhound* at Plymouth, on July 29, 1865, in which she had the same draught as that styled "normal displacement and trim" in these experiments. I have also imported into the comparisons a trial of the ship *Mutine*, sister ship of the *Greyhound*, on February 9, 1865, at precisely the same displacement.

Ship.	Speed on measured mile, in feet per minute.	Resistance due to speed.*	Effective horse-power, *i. e.* resistance. $\times \frac{\text{Velocity}}{33,000}$.	Actual indicated horse-power on trial.	$\frac{\text{Effective horse-power.}}{\text{Actual indicated horse-power.}}$
Greyhound	1,017	10,770	332.1	786	.422
	845	6,200	158.7	453	.350
Mutine	977	9,440	279.5	770	.363
	757	4,770	109.4	328	.334

*Deduced from the towing experiments with *Greyhound*, including an estimate of air-resistance of masts and rigging.

From this it would appear that the engine and propeller efficiency of these ships is less at low speed than at full speed, and that at the best there is a loss of 58 per cent.

A perhaps more instructive way of treating the question is to compare the apparent thrust of the propeller with the actual resistance of the ship; and this comparison appears in the following table:

Ship.	I. H. P. on measured mile trail.	Speed of screw, in feet, per minute.	Indicated thrust in pounds *i. e.* $\frac{\text{I. H. P.} \times 33{,}000}{\text{speed of screw}}$	True resistance in pounds deduced from towing experiments.	$\frac{\text{True resistance.}}{\text{Indicated thrust.}}$
Greyhound..	786	1,245	20,830	10,770	.517
	453	1,039	14,390	6,200	.431
Mutine......	770	1,230	20,650	9,440	.457
	328	952	11,380	4,770	.419

Making the utmost allowance for engine-friction, etc., it seems from this impossible to doubt that the actual thrust delivered by the screw-shaft is largely in excess of the resistance due to the ship, and that considerable extra resistance must be caused to the ship by the action of the screw by the diminution which that action produces on the hydrostatic (or perhaps I should say hydrodynamic) pressure of the water against the contiguous parts of her run. I have often insisted on this effect of the screw working in the "dead water" close to a ship's stern—although, considering the great importance of the subject, I have had but small success in my endeavors to draw attention to it, and I believe that there is a general supposition that the effect is but small in the case of a ship having as fairly fine a run as the *Greyhound*. Nevertheless the above comparison seems to show that it is considerable; and this is corroborated by six of the towing experiments at "normal trim and displacement" (shown in Fig. 6, Plate IV), taken with the screw lowered into its working position. In three of these six experiments, the screw was allowed to revolve; and it will be seen that in these three cases the resistance of the ship is much higher than at the same speeds with the screw lifted, nay even than with it down and fixed.

In order to test the "scale of comparison" which has been propounded by me as furnishing a true method of inferring the resistances of a ship from those of a model of the ship, a model of the *Greyhound* $\frac{1}{16}$ full size was made, and its resistances at various speeds determined under each of the different conditions of displacement and trim to which the ship herself was subjected. This was done in the experiment tank, and with the apparatus constructed by me for the experiments I am now carrying on with models for the Admiralty.

The results of these experiments on the *Greyhound* model are shown in Fig. 17 (Plate VII), in the same form as that in which the results with the full-sized ships are exhibited in Figs. 7 and 8. These results of the experiments with the model, being free from the prominent causes of error involved in the full-sized trials, were obtained with considerably greater accuracy; and consequently the differences of resistance developed at different trims may be inferred from these with tolerable precision; and it is at a glance observable that these differences are generally in the same direction as those deduced from the full-sized trials, and that there is a great resemblance in character between the "curves of resistance" of the model and of the ship.

But in order to test this correctly it is necessary to apply the definite "law of comparison," which may be thus stated: If the ship be D times the "dimension" (as it is termed) of the model, and if at the speeds V_1, V_2, V_3 . . . . the measured resistances of the model are R_1, R_2, R_3 . . . . , then for speeds $\sqrt{D}V_1$, $\sqrt{D}V_2$, $\sqrt{D}V_3$ . . . . of the ship, the resistances will be D^3R_1, D^3R_2, D^3R_3 . . . . To the speeds of model and ship thus related it is convenient to apply the term "corresponding speeds." This law would certainly hold good according to the old rule that the resistance varies as the square of the velocity, and again as the area of the surface exposed to resistance, or as that of the midship section—a law which has been generally held to express accurately the resistance due to surface-friction, and the formation of dead-water eddies, of which the wake of a plane moving at right angles to itself may be regarded as the most perfect example; and, as will be presently seen, there is a great reason to conclude that almost the only element of resistance over and beyond these is that due to the formation of the waves which the passage of the ship creates. These waves are undoubtedly originated by the differences of hydrodynamic pressure inherent in the system of "stream-line motion" which accompanies the ship; and, according to theory, when the originating forms are similar, and travel at speeds proportional to the square roots of their respective dimensions, the resulting forces, being as the squares of the speeds, will be such as to create wave configurations precisely similar in every respect. That is to say, if for instance the surface of the water surrounding a ship 160 feet long, traveling at 10 knots, were modeled together with the ship on any scale, the model would equally represent, on half that scale, the water surface surrounding a ship of similar form 320 feet long, traveling at 14.14 knots, or, again, on sixteen times that scale the water-surface surrounding a model of the ship 10 feet long, traveling at 2½ knots. This being so, it follows that the resistance caused to these forms respectively by the development of the waves would be proportionate to the cubes of the dimensions of the forms, and would therefore strictly follow the law of comparison already quoted. A confirmation of this proposition of the similarity of the waves caused by similar forms traveling at corresponding speeds was incidentally afforded by the experiments made by me for the Admiralty in July, 1872, on the form proposed by Mr. Ramus, in which two similar models of greatly different dimensions were tried at various speeds. The configurations of the water-surfaces in contact with the models were carefully noted in every case, and were found to accord precisely with the above theory; and diagrams exhibiting this were sent in with the report upon Mr. Ramus's proposal. With the *Greyhound* model also, the resemblance to the waves developed by the ship at corresponding speeds was most striking, even to the peculiar features of the surge at the bow.

The "law of comparison," then, would be absolutely correct if the elementary resistances due to wave-making, to surface-friction, and to the formation of dead-water eddies constituted the entire resistance, and if, as has been generally believed, it were strictly true of the latter two elements alike that the resistance varies as the square of the speed and as the area of the surface on which it acts. With reference to dead-water eddies, indeed, this double proposition may be confidently accepted; but the experiments on surface-friction, of which a report has been sent in by me to the Admiralty, show that, in regard to this latter elements at least, the proposition does not express the exact truth. In fact, in dealing with surfaces having so great a disparity in length and speed as those of a model and of a ship, a very tangible correction is necessary; but it is one of easy application, and the data afforded by the friction experiments are so definite that there is practically no room for error in its application, given the nature of the surface of the ship. Unfortunately that is to some extent an unknown quantity in the case of the *Greyhound;* indeed, the differences that may be caused by difference in quality of surface being very considerable, the absolute resistance of any ship is an indeterminate quantity, and thus the test of the law of comparison which the full-sized trial affords proves less definite than might be wished, and it is desirable to trace out the limits of the indefiniteness.

To compare conclusively the resistance-curves of the ship and of the model, the best representative we can select of each will be an average of the curves which give, for each, the results at the several trims; and proceeding on this assumption, the corrective data which the experiments on surface-friction supply may be introduced into the comparison as follows: In Fig. 18 (Plate VII) the ordinates of the line A A show the resistance at various speeds of the model of the *Greyhound* at normal displacement, being an average of the resistance at different trims. Those of the line B B show the resistance of the model due to surface-friction alone, calculated from the experiments on the supposition that the quality of the ship's skin is equivalent to what became a serviceable standard of quality in those experiments, namely, that of smooth shellac varnish*; consequently the remainders of the ordinates

* For this calculation the immersed skin was carefully measured, and the resistance due to it determined upon the hypothesis that it is equivalent to that of a rectangular surface of equal area, and of length (in the line of motion) equal to that of the model, moving at the same speed. I am confident that no sensible error arises from thus disregarding the small alternate motions in the surrounding water due to stream-line action.

(*i. e.*, the parts included between line A A and line B B) express the resistance due to other causes than surface-friction; and to these, it seems certain, the law of comparison correctly applies; hence the portions of the ordinates which are included between the two lines A A and B B represent correctly, when interpreted by the scales appropriate to the ship, the resistance of the ship without surface-friction. The resistance of the ship due to surface-friction is then calculated in the same way as that of the model; and it is represented by the line C C, measuring the ordinates downwards from the line B B on the appropriate scales. Then the ordinates of the curve A A, measured similarly from the curve C C instead of from the base, represent the total resistance of the ship as deducible from that of the model.

The corrected resistance curve thus deduced for the ship represents, it should be mentioned, her resistance in *fresh water* (the models are tried in fresh water), and must be appropriately corrected for a comparison with her sea-going trials. According to the theory on which the law of comparison is based, the corresponding speeds will be the same for either salt water or fresh; therefore no modification is required in the *speed*-scale. But the resistance due to *wave making* will be, at any given speed, precisely proportionate to the density; and I have found, by an experiment on the flow of water through a long pipe, that the *frictional resistance* for a given speed also varies as the density; consequently the fresh-water resistances must be increased in the proportion of the density of salt water to fresh. In Fig. 19 the resistances thus finally deduced for the ship from the model are shown again by the line A A, while those obtained from the trial of the ship herself are shown by the line B B; and it will be seen that the latter are in excess.

As has been already mentioned, it is not easy to ascertain the resisting quality of the *Greyhound's* surface with precision; but it is not improbable that her copper, deteriorated by age, possessed a decidedly worse quality of surface than fresh varnish, for which the curve shown is estimated; and this circumstance may account for the excess which the resistance of the ship herself thus exhibits.

The excess may be instructively qualified by saying that it is about equivalent to what would have resulted if the surface assumed for the ship, instead of consisting of fresh varnish throughout, consisted of a skin of ordinary unbleached calico for one-third of its area—that is to say, for an area equivalent to the surface of the keel with a strip 5 feet wide on each side of it throughout the length of the ship. The result calculated on this hypothesis is exhibited by line C C, Fig. 19 (Plate VII).

This explanation shows that a quality of surface which it requires no violent supposition to attribute to the ship would fully explain what, at first sight, appears inconclusive in the actual comparison between the ship and the model, regarded as a test of the law of comparison.

It is obvious, indeed, that there is no special fitness in the comparison between the old copper of an old ship and a smooth surface varnished with shellac; and the latter was only selected because its easily-secured uniformity of quality rendered it suitable as a standard among those I had subjected to experiment; and, on the other hand, there is no violence in the supposition that, if the series of experiments on surface-friction had included a surface of a quality counterpart to that of the *Greyhound's* copper, its coefficient of resistance would have been equivalent to two-thirds varnish and one-third unbleached calico.

The comparison therefore between the *Greyhound* ship and the *Greyhound* model certainly throws no doubt on, if it does not conclusively verify, the law of comparison between ships and models, the discrepancy which it presents being only such as might arise in comparing the performances of any given ship under two different conditions of skin. And I may, not unfitly, recapitulate here, in confirmation of the law, the following considerations which appear to me to demonstrate that (taken as including the definite corrections which have been assigned to it) it overlooks no real element of resistance, and correctly qualifies the elements which it in terms contemplates. Of these three elements, only two are experienced by a body wholly submerged—namely, the drag of dead-water eddies and surface-friction; and, indeed, according to the improved perception of the nature of fluid-resistance which the theory of stream-lines has supplied, these two elements constitute the entire resistance of a totally submerged body, and may be taken as typical of fluid-resistance proper. Now, it has been already pointed out that the former operates in exact conformity with the law of comparison, and that the latter operates in approximate conformity with it, being also amenable to determinate corrections, which render the conformity in effect exact. This establishes the law for a body wholly submerged.

Again, on transferring the path of the resisting body to the surface, the dynamic conditions of the surface obviously introduce one prominent new element into the constitution of the resistance—namely, the performance of work in the formation of waves; now there seems no room for error in the general reasoning by which it was shown that this element also would conform to the law of comparison.

Moreover, there is no reason whatever to suppose that the proximity of the surface introduces any other new element of resistance of appreciable magnitude; and the conclusion that it does in fact introduce no other is strongly supported by an analysis of the resistance-curves taken in connection with the initiation and growth of wave-development, regarded as a function of the speed of the ship or model.

Thus, in Fig. 18, it is to be seen that at a speed of 100 feet per minute the line A A, which represents the recorded resistance of the model, nearly coincides with the line B B, which shows the calculated resistance due to her surface-friction alone; and this implies that up to that speed almost the whole resistance is accounted for by surface-friction, the small residuum being probably due to the drag of dead-water eddies.

Now, up to this speed it was plainly visible that the model traveled without producing sensible waves.

As we proceed further and further along the scale of speed we find that the two lines diverge increasingly, showing that, as the speed increases, the entire resistance becomes more and more in excess of that due to surface-friction.

But here also the wave phenomena were in accordance with those of the resistance; for the formation of waves of sensible magnitude became apparent just at the speed at which the excess of resistance becomes marked by the divergence of the lines, and to all appearance the increase in the magnitude of the waves exactly kept pace with the increase in the excess of the resistance; thus it is impossible not to attribute the growth of the excess to the growth of the waves. Coupling this visible justification of the principle on which the law of comparison is founded with the circumstances that the law is not open to any known objection, and that, moreover, a very rational assumption as to the quality of the *Greyhound's* surface would render the results of her trial a conspicuous verification of it, there appears to me ample reason for accepting it as the true law.

SUMMARY AND REMARKS.

To sum up shortly the results of this investigation—

1. The method of conducting the experiments with the full-sized ship may be considered to have been in almost all respects very successful, especially considering the novelty and magnitude of the work.

The expedient of towing from a long outrigged boom, so as to be clear of wake disturbance, answered perfectly—the *Active* (3,078 tons, 4,015 H. P., 15 knots measured-mile speed) towing the *Greyhound* (1,157 tons) at nearly 13 knots speed, from the end of a boom 45 feet long, without any difficulty in steering. A perfectly straight course of each ship was skillfully kept by the officers in charge.

The dynamometer, with its arrangements of towing-truck, etc., was in all respects successful. This instrument is at Portsmouth, and, should occasion require, may be usefully employed for similar or kindred experiments.

If the apparatus were to be used again for towing ships, a more compact appliance for relieving the dynamometer of sudden strains should be devised in place of the somewhat rude though effective arrangement of relieving-rockers I adopted in the *Greyhound*.

The arrangements for recording the speed were less successful than there seemed reason to hope, chiefly in consequence of the irregularities of tidal currents.

In any further towing-trials that might be made, I should recommend the adoption of a powerful screw-log which should record its revolutions on the diagram, together with a Berthon log, for the convenience of noting promptly variations in speed.

While the arrangements on the whole answered well, my experience in the conduct of these experiments fully bore out the views I had previously expressed, of the almost impossibility of entering on a comprehensive investigation of the properties of different forms of ships by full-size towing-trials.

In the trials with the *Greyhound* we had the good fortune to meet with six weeks of almost uninterrupted fine weather.

2. With regard to the results of the experiments as elucidating the performances of ships such as the *Greyhound:*

a. The actual amount of towing-strain for the *Greyhound* (which is interesting as exhibiting the small amount of resistance that ships offer) was approximately as follows:

	Tons.
At 4 knots	0.6
At 6 knots	1.4
At 8 knots	2.5
At 10 knots	4.7
At 12 knots	9.0

b. A comparison between the indicated horse-power of the *Greyhound*, when on her steam trials, and resistance of the ship, as determined by the dynamometer, shows that, making allowance for the slip of the screw, which is a legitimate expenditure of power, only about 45 per cent. of the power exerted by the steam is usefully employed in propelling the ship, and that no less than 58 per cent. is wasted in friction of engines and screw and in the detrimental reaction of the propeller on the stream-lines of the water closing in around the stern of the vessel.* Thus there appears to be an ample field for improvement in the propulsion of vessels.

c. Altering the fore-and-aft trim of the ship appeared to show that no very great difference in the vessel's resistance when under steam would be effected by ordinary changes in her trim.

d. Lightening and so diminishing the displacement of the ship did not seem in the case of the *Greyhound* to be proportionally advantageous. This result, so far as it goes, indicates a superiority as regards resistance in deep rather than broad ships.

e. The screw (two-bladed), when lowered and revolving freely, gave even a greater resistance than when fixed with the blades upright.

f. The addition of bilge-keels of considerable size (100 feet long and

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*This last-mentioned cause of waste in the propulsion of ships is one to which I have for a long time past repeatedly called attention. I investigated it in small-scale experiments some years ago; and my views have received great confirmation from the experiments with the *Greyhound*. In the series of experiments I am conducting for the Admiralty I hope to be able to introduce arrangements by which these points may be crucially tested. The subject is one of immense importance; for making every allowance for the power employed in overcoming friction of engines and screw, there remains in the case of the *Greyhound* some 40 per cent. of waste, an amount the true cause of which is certainly worthy of investigation.

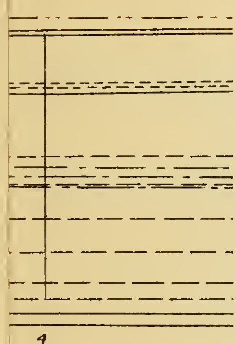
3 feet 6 inches broad) produced an increase of resistance less than there appeared good reason to anticipate and unimportant compared with the total resistance of the vessel.

3. As I have not been able myself to construct or to appreciate any general expression which has seemed to me to be even theoretically satisfactory for determining *a priori* the resistance of any given form of ship, I am not in a position to point out how far the experiments with the *Greyhound* are of use either in determining the validity of such formulæ or interpreting their co-efficients.

4. The experiments with the ship, when compared with those tried here with her model, substantially verify the law of comparison which has been propounded by me as governing the relation between the resistances of ships and their models.

This justifies the reliance I have placed on the method of investigating the effects of variation of form by trials with varied models—a method which, if trustworthy, is equally serviceable for testing abstract formulæ or for feeling the way towards perfection by a strictly inductive process.

W. Froude.


4


50
40
4

Sheer Drawing of H.M.S. "Greyhound."

Fig 1.

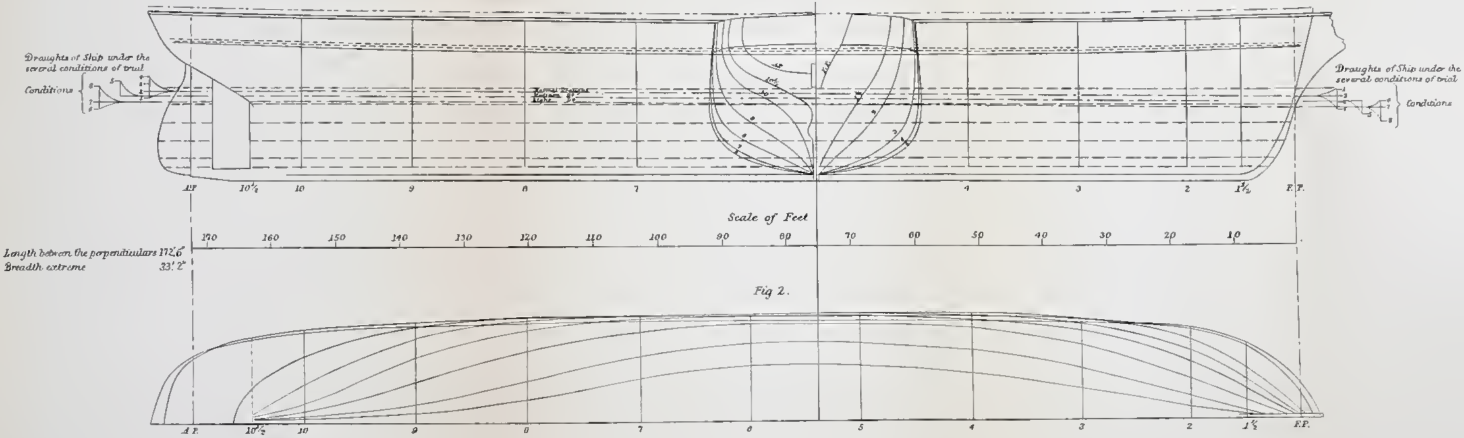
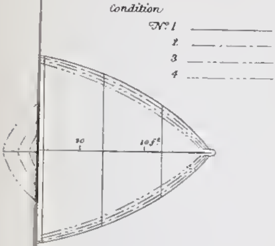
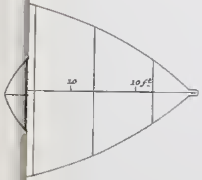


Plate II.





Water-line of H.M.S. "Greyhound" under the several Conditions of Draught and Trim.

Fig. 3. & Fig. 4.

Fig. 3. Water lines in Conditions No. 1. 2. 3. & 4.

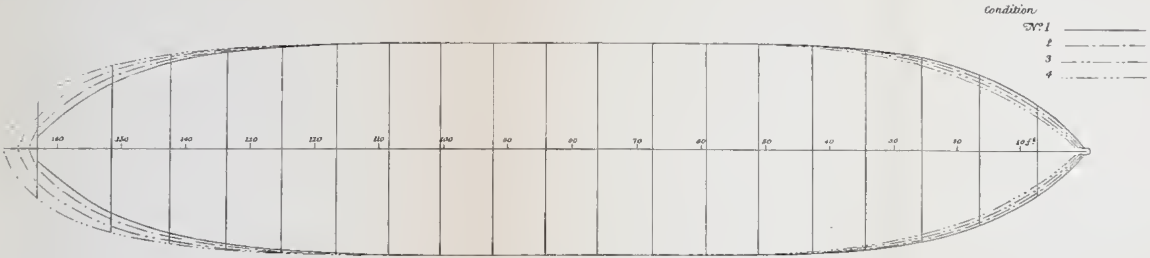


Fig. 4. Water-line in Condition No. 5.

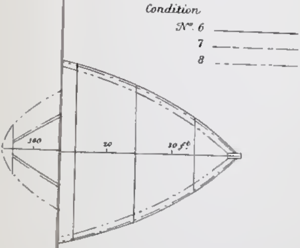
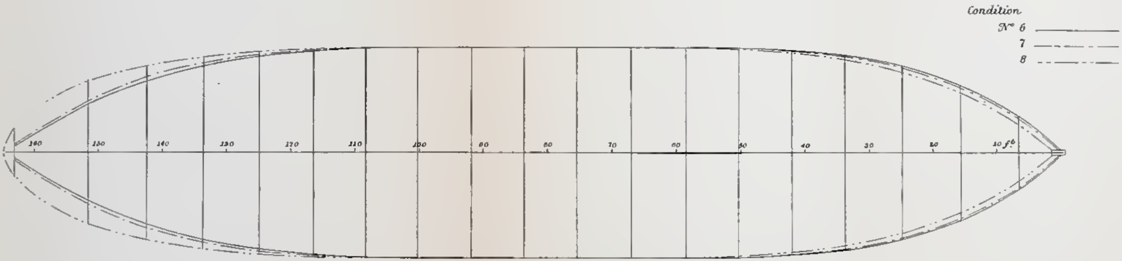
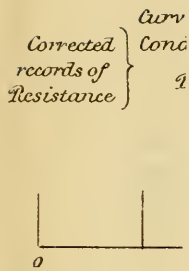

Condition
N°. 6
7
8
140
20
30 ft.

Plate III.

Water-line of H.M.S. "Greyhound" under the several Conditions of Draught and Trim.

Fig. 5. Water-line in Conditions No.s 6, 7, & 8.




Curv
Corrected records of Resistance
Cond
0

Curves of Resistance of H.M.S. "Greyhound" under the several conditions of Draught & Trim

Fig. 6 to Fig 9

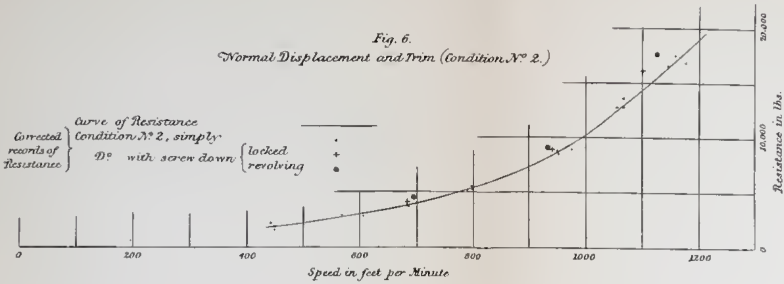


Fig. 6. Normal Displacement and Trim (Condition No. 2.)

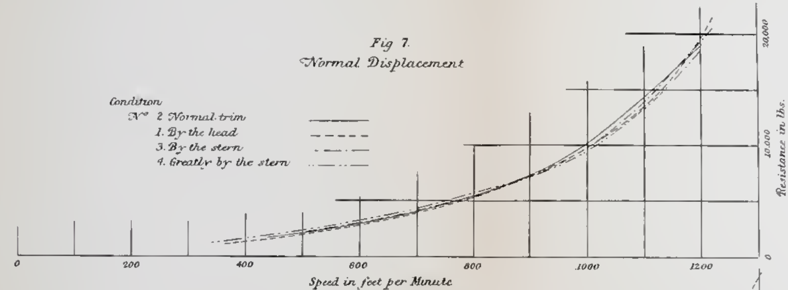


Fig 7. Normal Displacement

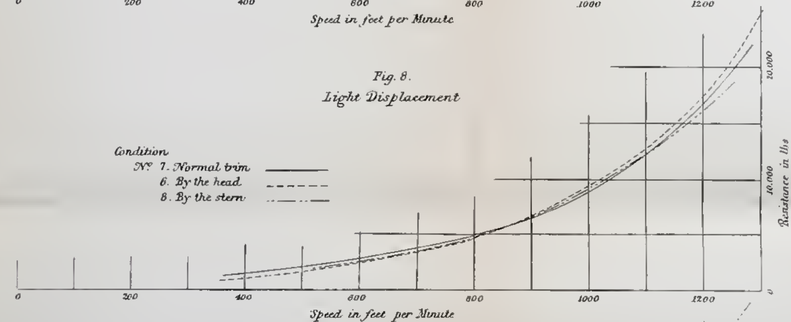


Fig. 8. Light Displacement

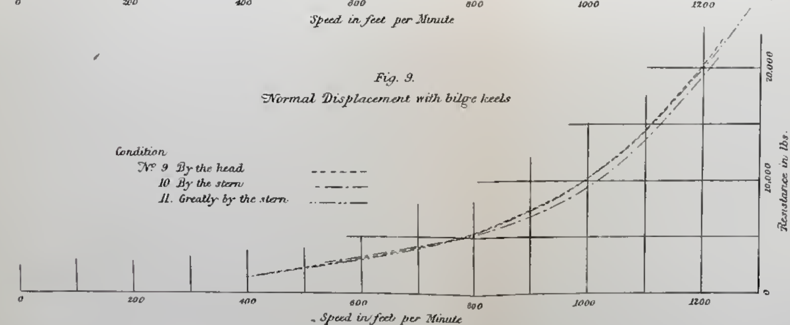


Fig. 9. Normal Displacement with bilge keels

Curv
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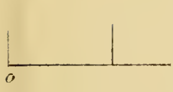

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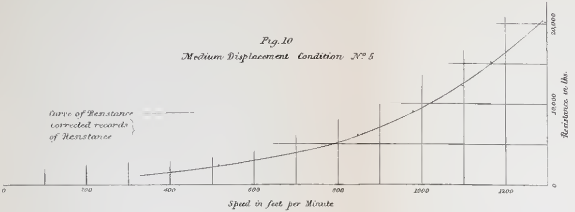
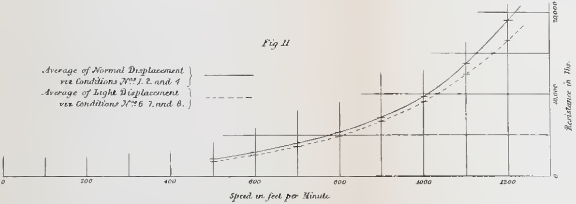
Curves of Resistance of H.M.S. "Greyhound" under the several conditions of Draught and Trim

Fig. 10. to Fig. 12.





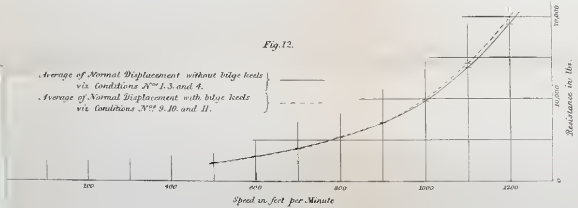


Plate. VI.

Diagrams of Retardation of H.M.S. "Greyhound."

Figs 13 & 14.

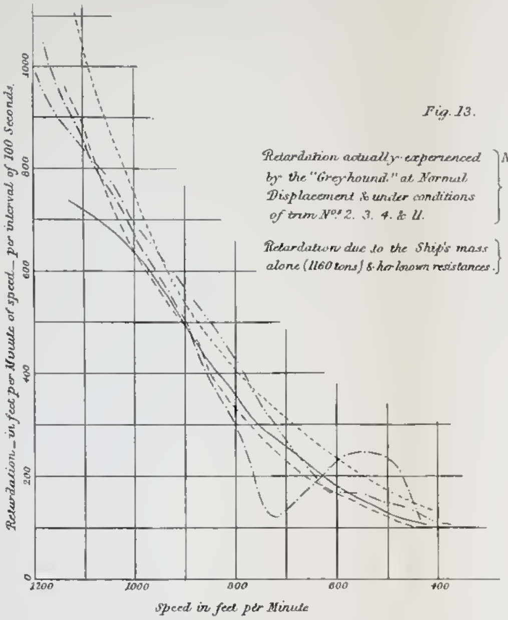


Fig. 13.

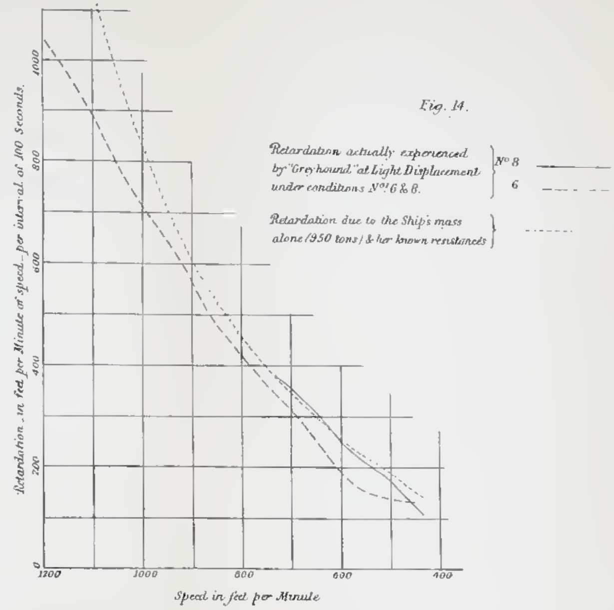


Fig. 14.

Figs 15 & 16 __ Diagrams of acceleration of H.M.S. "Greyhound."

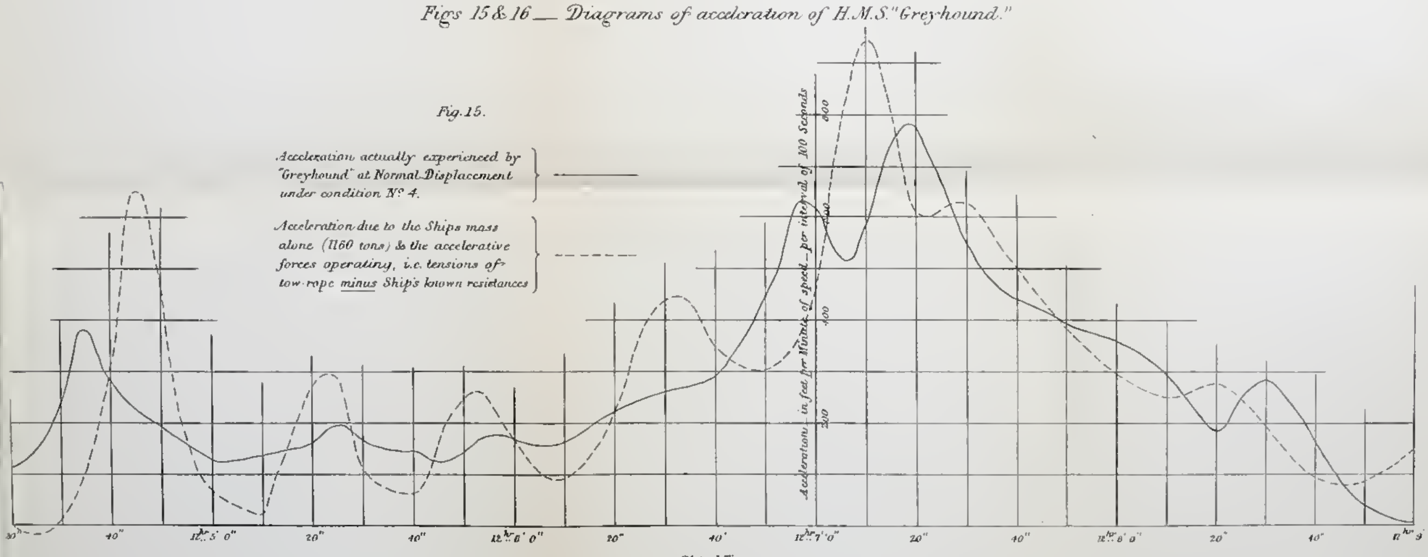


Fig. 15.

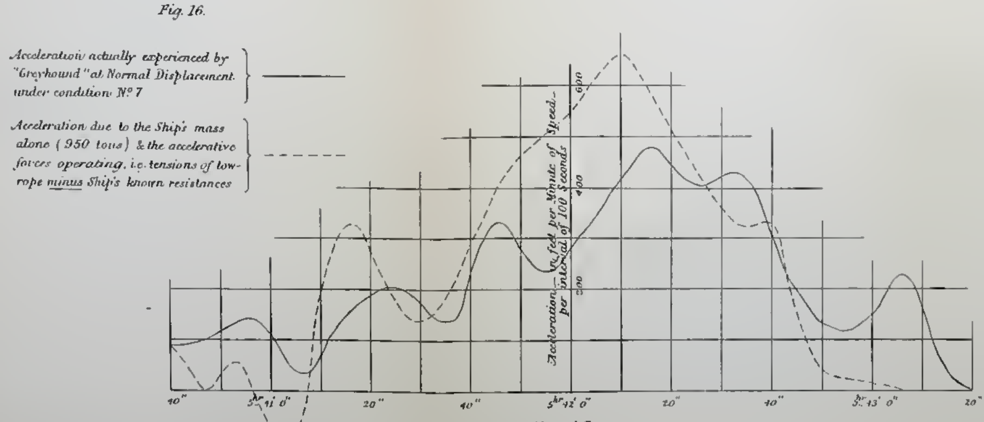
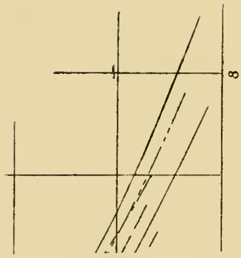


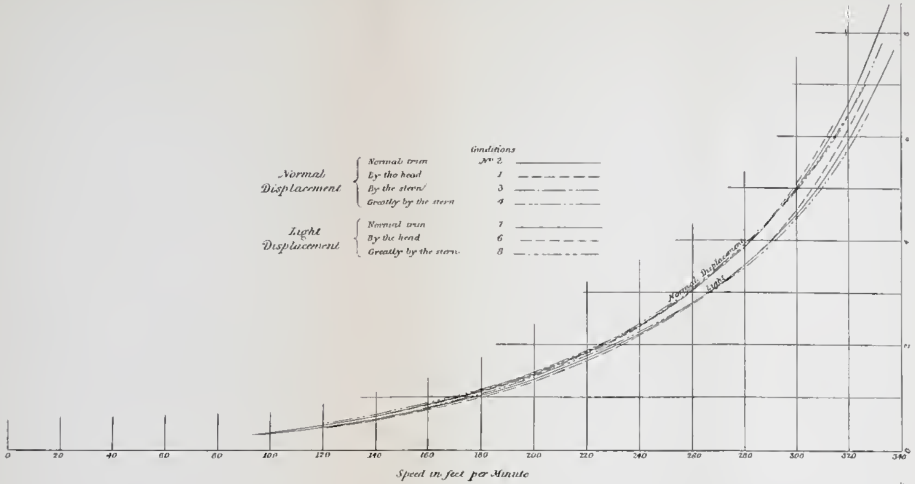
Fig. 16.

Plate VII.



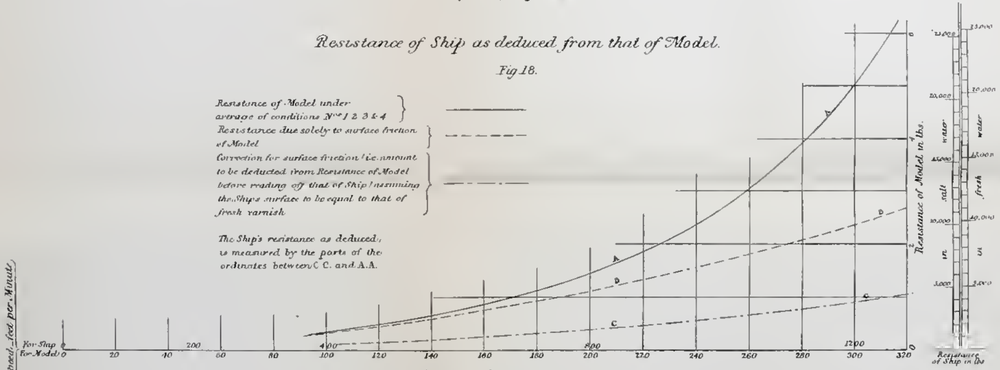
Resistance of "Greyhound Model" under conditions of Draught & Trim similar to those of H.M.S. "Greyhound" _ Scale of Model 1/16

Figs 17.



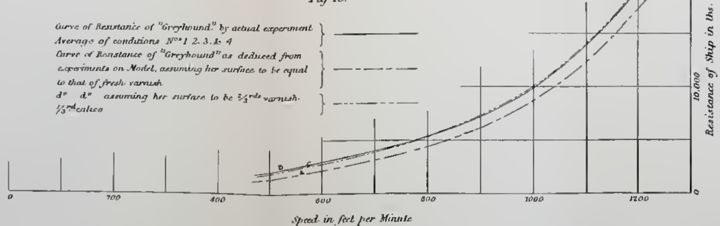
Resistance of Ship as deduced from that of Model.

Fig 18.



Actual Resistance of Ship compared with that above deduced from Model

Fig 19.



2.

COMPARATIVE RESISTANCES OF LONG SHIPS OF SEVERAL TYPES.

By W. Froude, Esq., M. A., F. R. S.

[Read at the Seventeenth Session of the Institution of Naval Architects, 7th April, 1876.]

The trial of the model of Mr. Denny's ship *Merkara*, referred to in the paper I have already read, furnished materials for extending and giving practical completeness to a comparison which our series of experiments had already led us to institute between several types of form.

The comparison is interesting, as showing the relation between length and resistance with two types of form—(1) That form in which a straight parallel-sided middle body is interpolated between two ends of greater or less fineness; (2) that in which the whole length of the ship is utilized in fineness of form; the results being worked out for the *Merkara* and three other ships of the same displacement, but of different form and proportions. The lines of these three other forms selected are those which in our series of experiments have been found to give, on the whole, the best results within ordinary available limits of speed. The displacement in each case is 3,980 tons, which was that of the *Merkara* on her trial. For shortness, I shall call the four models A, B, C, and D; A being the *Merkara*. A and D represent No. 1 type, but with different proportions and different degrees of fineness; B and C represent type No. 2. They have different proportions but the same degree of fineness. The ends of D beyond the parallel middle body have the same fineness as the ends of B and C; indeed, all these forms have what may be called the same entrance and the same run, for, though their dimensions as well as their proportions of length to beam differ, the cross-sections on which their lines are based are throughout the same, the longitudinal spacing being in each case made proportional to the total length they occupy, or as it may be termed, having different degrees of expansion. The table on next page gives the leading particulars of the four ships.

The length as given does not include the screw aperture, taken to be 9 feet. In A the entrance and run are each 3.87 beams; the total length is 9.67 beams; the draught is .436 of the beam. In D the entrance and run are each 2.08 beams; the total length is 6.25 beams; the draught is .392 of the beam. In B and C the total length is 7.82 beams and 6.26 beams, respectively; in both the draught is .392 of the beam.

Type.	Displacement.	Length.				Extreme breadth.	Mean draught.	Area of skin.	Square root of entrance plus run.
		Entrance.	Parallel middle body.	Run.	Total.				
		Feet.	*Feet.*	*Feet.*	*Feet.*	*Feet.*	*Sq. ft.*		
A	3,980	144	72	144	360	37.2	16.25	18,660	17.03
B	3,980	179.5	....	179.5	359	45.88	18	19,130	18.95
C	3,980	154.5	....	154.5	309	49.4	19.32	17,810	17.58
D	3,980	95	95	95	285	45.56	17.89	16,950	13.78

These explanations are a sufficient introduction to the consideration of the several Figs. 1, 2, 3, and 4 (Plate I), which give the lines of the several ships, and Plate II, which gives their respective curves of resistance; that is to say, curves in which the abscissæ represent speed in knots, and the ordinates, resistance in pounds. In studying Fig. 5, perhaps the circumstance which first deserves notice is that at the maximum speed included in the steam trials of the *Merkara* there is so extremely small a difference between the resistance of these four ships, differing so considerably as they do in form and proportions. Making the comparison at 12 knots, the greatest of the resistances, that of D, is 21,000 pounds; the smallest, that of C, is 18,700 pounds; the difference being only 2,400 pounds, or scarcely over 10 per cent. An interesting light under which to view this amount of difference is that thrown on it by the fact that it is only half of the thrust equivalent to the constant or initial friction of the *Merkara's* engines, a circumstance which indicates how easily a difference in the performance of the ship may be masked by a difference in the performance of the engines. If, in comparing the ships at this speed, we omit D, as possessing considerably less sharp lines of entrance and run than either of the other three, the differences between the resistances are still smaller, the 20,000 pounds of the *Merkara* exceeding the 18,700 of C by only 1,400 pounds, or 7.5 per cent.; and even at 13 knots, which just exceeds the highest speed included in the *Merkara* steam trials, though the resistance of D has become rather more in excess, yet that of the *Merkara* and B are identical, and that of C, the smallest of the three, is still only 1,400 pounds below theirs. At 13½ knots the resistance of D is beginning to diverge rapidly into excess, and the *Merkara's* is also beginning to diverge in the same direction, though less rapidly. At still higher speeds the forms C and B, which have no parallel middle body, show a growing superiority to the *Merkara*, though the longest of them is only as long as that ship, and is nearly 8 feet broader, while C, the shorter of the two, is 51 feet shorter, and at the same time 12 feet broader than the *Merkara*. Up to 16 knots the resistance of C is less than that of D, though C is 4 feet the broader and 24 feet the shorter.

The results of the comparison are interesting in their relation to a proposition which I have elsewhere insisted on, namely, that at very low speeds (speeds low as compared with the length of the ship) a ship's resistance, if her form be fairly fine, consists practically of nothing beyond surface friction; and it is worth while to notice how far the proposition is verified in the case before us. The model of the *Merkara* was tied down to a speed lower than we used formerly to include; so that at the lowest speed which her curve of resistance includes I have not data for the verification of the proposition with the other forms; but it is the fact that in the *Merkara's* case at from 5 knots to 8 knots the surface friction is about 92 per cent. of the ship's entire resistance. And referring to plate II, we see that although at 9 knots D, with a less skin, has a greater resistance than the *Merkara*, yet the line of her resistance-curve is converging on the *Merkara's*, and might be expected to cross it at a still lower speed; B, however, which has a skin area rather larger than the *Merkara's*, has, nevertheless, rather the smaller resistance of the two; but C, which has the smallest skin area of all, has also the smallest resistance.

A more practical aspect of the comparison may be made in relation to higher speed; and even in reference to a speed as high as 12 knots it will be seen how valuable a diminished area of skin may be when the effect of fouling is taken into consideration, a circumstance of especial importance to a ship of war which may be under the necessity of keeping the sea for long consecutive periods. I am not able to say how much fouling exactly is to be anticipated thus under any given circumstances, nor again can I say exactly how much extra resistance a given amount of fouling will produce. But our experiments on surface friction show that the substitution of a surface of ordinary unbleached calico for one of clean paint produces just a double frictional resistance, and a foul bottom must often be no less obstructive. For simplicity of calculation I shall suppose a degree of fouling by which the resistance is exactly doubled, and it will be seen that the effect of this is to improve greatly the relative positions of the shorter ships. The construction of the table sufficiently explains itself.

It will be seen that under these circumstances D, which in the comparison as it stands with the clean skin is the worst of the four, having a resistance exceeding that of the mean of the two long ships in the ratio of nearly 1.1 to 1, has become as good as the best of the two, now that the skin resistance is doubled, and under the same circumstances the superiority of C has been notably increased.

Names of ship.	Area of skin.	Total resistance.	Resistance due to skin friction alone.	Residuary resistance independent of skin.	Doubled skin friction.	Total resistance with skin friction doubled.	Remarks.
A Merkara	18,660	20,000	15,600	4,400	31,200	35,600	Assumed speed, 12 knots.
B	19,130	19,220	16,160	3,060	32,320	35,380	
C	17,810	18,700	14,890	3,810	29,780	33,590	
D	16,950	21,100	14,250	6,850	28,500	35,350	

The superiority of the shorter ships in respect of handiness need hardly be referred to. Moreover, I can not but believe that the weight of hull constructionally necessary to the strength of the ship must be considerably less for the shorter than for the longer ship. Mr. Denny, indeed, is not, I believe, prepared to admit this; but the question raised is one which I think must be capable of something like a general and almost demonstrative solution, and it is one which ought to be treated on general mechanical principles, and not decided by mere reference to the existing rules of Lloyds. These, however, are, I trust, undergoing revision in very capable hands.

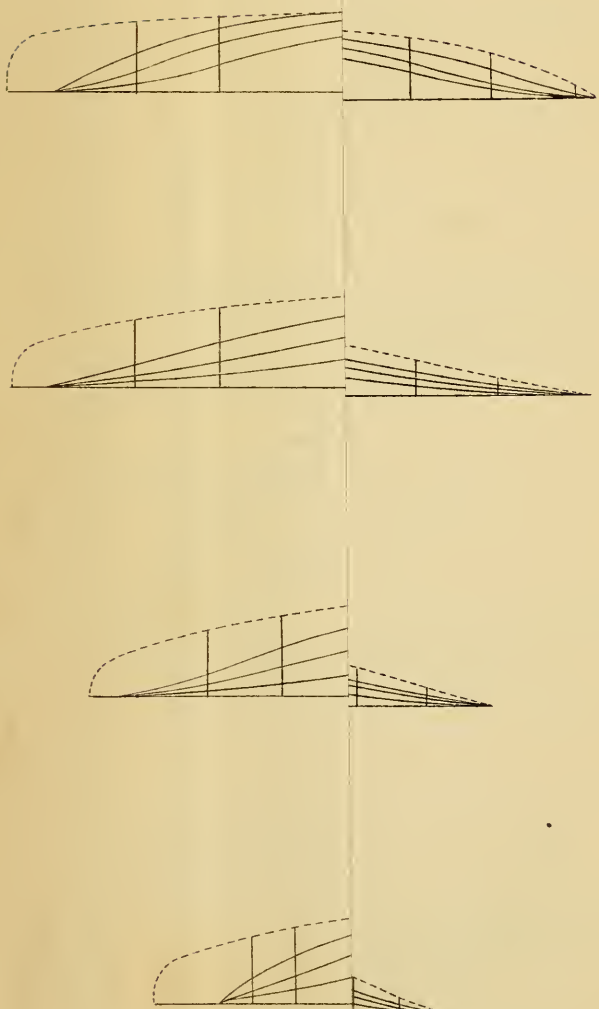
Nevertheless, it must be admitted that in view of the importance of large carrying power, combined with limited draught—a limitation which the Suez Canal has done much to emphasize—and I may add, in view of the practical sufficiency of what may be called "moderate speed," the prevailing tendency to great length, including a long parallel middle body, is a fair result of "natural selection;" and this form, if rationally treated, is perhaps, under the conditions indicated, the best adapted for commercial success, though, where deep draught is unobjectionable, a shortened form with no parallel middle would be, as I have shown, unquestionably superior; or were it an object to obtain very high speed, without notable increase of resistance, parallelism of middle body would, even with the longer form, be inadmissible.

The logic of the circumstances shapes itself thus: Large displacement means large dimensions, somehow or somewhere; but the limitation of draught forbids enlargement of dimension except in the direction of length, since increased ratio of breadth to depth would involve an objectionably raised metacenter and objectionable increase of skin; greatly extended length has, therefore, for mercantile purposes, become essential to large carrying power. Now, with a very long ship, if the ends are so far fined as in effect to limit the resistance to surface friction, the parallelism of the remainder clearly assigns a valuably increased carrying power to the ship as a whole; or, what comes to the same thing, secures a given carrying power with less total skin, and therefore less resistance at moderate speed. What I contend against is, not the parallel middle body *per se*, but the mistaken idea which to most minds forms the basis of its justification—the idea, namely, that to lengthen a ship by merely introducing a parallel middle involves no material increase of resistance, the supposition being that the middle thus added will follow unobstructed through the opening made in the water by the full-sized ends.

In conclusion, I must remark that the preformance of Mr. Denny's ship is somewhat better than previous experiments had led me to expect it would be; for I had not expected to find that, after taking account of the resistance due to surface friction, her residuary resistance would be barely less at 12 or 13 knots than that of B, a ship of the same length, and certainly of finer lines. Whether this rather unexpected goodness lies in some specialty in the lines of the entrance and run, or in the form

Plate 1.

To illustrate Types.



To illustrate Mr Froude's Paper on Comparative Resistance of Long Ships of Several Types.

Fig 1.—A

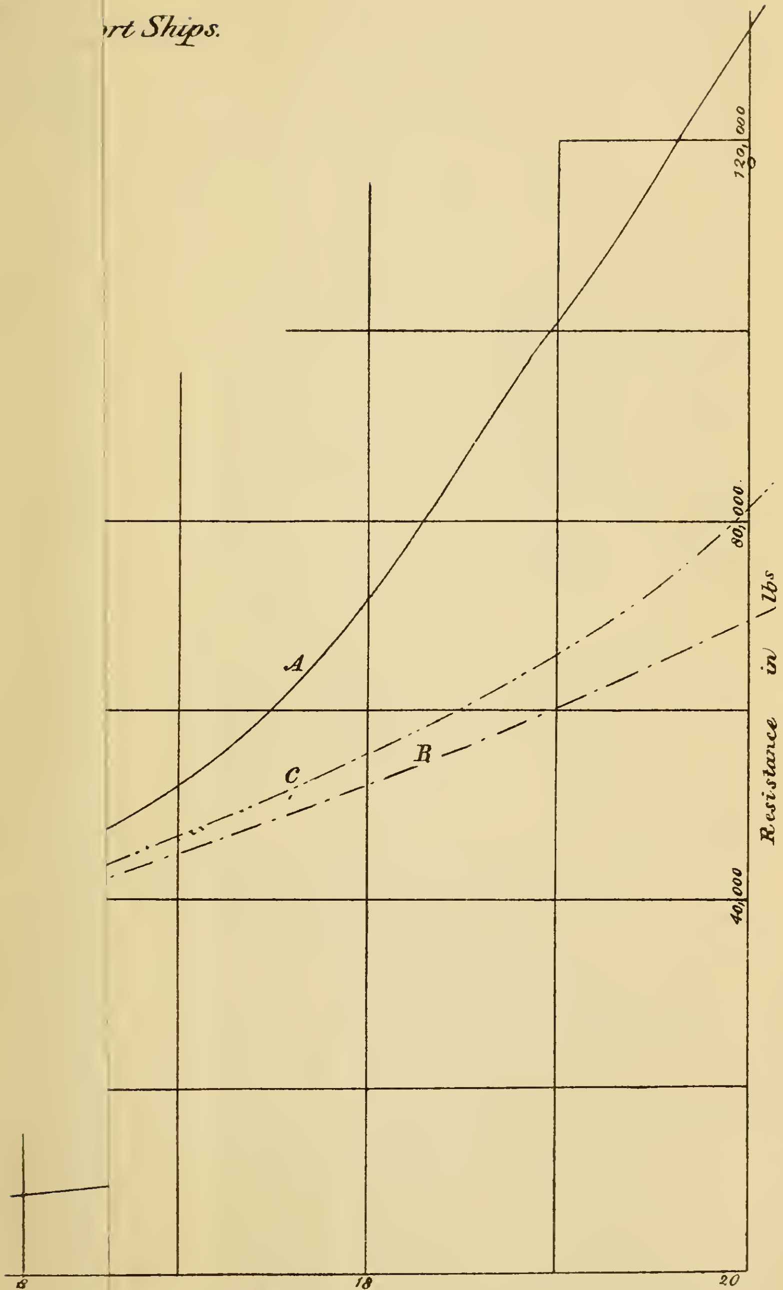
Fig 2.—B.

Fig. 3.—C

Fig. 4.—D.

Plate II.

rt Ships.

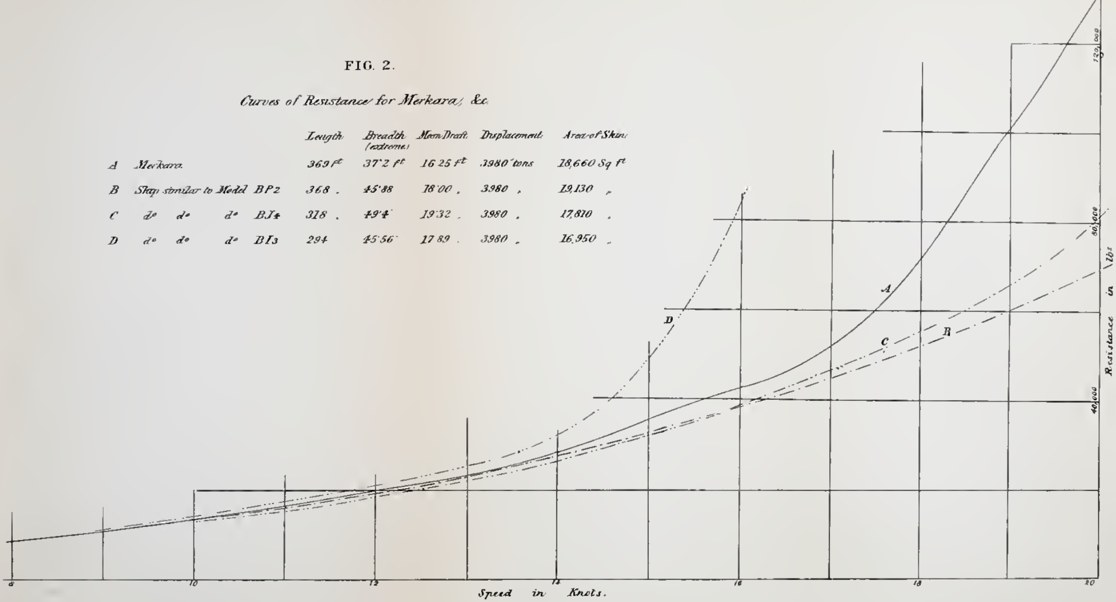


To illustrate Mr. W. Froude's Paper on Comparative Resistances of Long and Short Ships.

FIG. 2.

Curves of Resistance for Merkara, &c.

		Length	Breadth (extreme)	Mean Draft	Displacement	Area of Skin
A	Merkara	369 ft	37·2 ft	16·25 ft	3980 tons	18,660 Sq ft
B	Ship similar to Model BP2	368 "	45·88	18·00 "	3980 "	19,130 "
C	do do do BJ4	318 "	49·4	19·32 "	3980 "	17,810 "
D	do do do BI3	294	45·56	17·89 "	3980 "	16,950 "



of the midship section, or whether it merely indicates that at a speed so moderate compared with the length of the ship's entrance and run as 12 knots it matters little what the lines are, provided they do not fall short of a certain standard of fineness, is an important question which I shall not feel has been decisively answered till a series of experiments bearing on the point shall have been completed. Some additional steps in the series have been suggested by the present examination.

3.

FUNDAMENTAL PRINCIPLES OF THE RESISTANCE OF SHIPS.

W. FROUDE, Esq., F. R. S.

I propose to consider those principles of fluid motion which influence what is termed the "resistance" of ships. By the term resistance, I mean the opposing force which a ship experiences in its progress through the water. Considering how great an expenditure, whether of sail or steam-power, is involved in overcoming this resistance, it is clearly most important that its causes should be correctly appreciated.

This subject is a branch of the general question of the forces which act on a body moving through a fluid, and has within a comparatively recent period been placed in an entirely new light by what is commonly called the theory of stream-lines.

This theory as a whole involves mathematics of the highest order, reaching alike beyond my ken and my purpose; but so far as we shall have to employ it here, in considering the question of the resistance of ships, its principles are perfectly simple and are easily understood without the help of technical mathematics; and I will endeavor to explain the course which I have myself found most conducive to its apprehension.

In order, however, to show you clearly what light the theory of stream-lines has thrown on the question, I must first describe the old method of treating it, which is certainly at first sight the most natural one, and we shall thus see what germs of truth that method contained, and how far these were developed into false conclusions.

It is a crude but instinctive idea, that the resistance experienced, either by a ship, or by a submarine body such as a fish, moving through water, is due to the necessity of the body plowing or forcing or cleaving a passage for itself through the water; that it has to drive the water out of its way and then to draw it in again after itself.

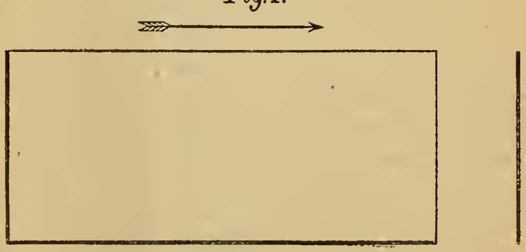
When, however, an attempt was made to deal with the matter in a scientific manner, it was seen that an explanation was needed of how it was that water required force to move it out of the way. For it may naturally be asked. How can there be reaction or resistance in a perfectly mobile material such as water seems to be? We can understand earth, for instance, resisting a plowshare dragged through it, and we can understand that even a perfectly thin, flat plane would make resistance if dragged edgeways through a sea of sand, or even through a sea of liquid mud, owing to the friction against its sides. But water appears, at first sight, altogether unlike this, and seems totally indifferent to change of form of any kind. If we stir water, the different currents seem to flow freely past one another, as if they would go on flowing almost forever without stopping. But we find, that although to push a thin oar blade through the water edgeways seems to require no force, yet, if we push it flatways, as in rowing, it offers a considerable reaction. The distinction, then, which suggests itself is that the particles of water, although they offer no resistance to anything merely sliding past them, offer great resistance to anything pushing against them, because the thing which is pushing against them sets them in motion out of its way, and to set anything heavy in motion, requires the exertion of force to overcome what is called its inertia.

This, then, appears prima facie to be the characteristic of water, that to set the particles in motion, or what is the same thing, to divert them from a straight path, requires force to overcome their inertia, although, when once set in motion, they are able to glide freely past one another, or past a smooth surface. This supposition embodies the natural conception of a fluid, and if it were absolutely exemplified in water, then water would be what we should call a perfectly frictionless fluid.

Now, though water is not absolutely frictionless, yet it is true that in many of the more familiar ways of handling it, the forces developed by its slight frictional qualities are small compared to those due to its inertia, and it is therefore not surprising that those who theorized on the resistance of ships thought it quite accurate enough to treat of the effect of the inertia only, and to neglect the comparatively small frictional qualities.

It was assumed, then, for the purposes of calculation, that the fluid being frictionless, would offer no resistance to a perfectly thin, flat, smooth plane, such as that shown in Fig. 1, moving edgeways through

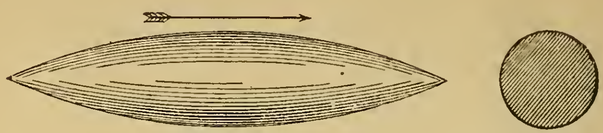
Fig. 1.



ELEVATION. CROSS-SECTION.

it, since this would in no way tend to set its particles in motion. But it is obvious that a ship, or fish, or other body, such as that shown in Fig. 2, moving through the water, has to be continually setting the particles of water in motion, in order, first, to get them out

Fig. 2.



ELEVATION. CROSS SECTION.

of its way, and afterwards to close them together again behind it, and that the inertia of the particles thus set in motion will supply forces reacting against the surface of the body. And it seemed certain, at first sight, that these reactions or forces on the surface of the body would necessarily so arrange themselves as to constitute resistance.

On this view, various formulæ were constructed by mathematicians to estimate these reactions, and to count up the sum total of resistance which they would cause to a ship or moving body of any given form. These formulæ were not all alike, but they were mostly based on the supposition that the entire forward part of the body had to exert pressure to give the particles motion outwards, and that the entire after-part had to exert suction to give them motion inwards, and that there was, in fact, what is termed *plus* pressure throughout the head end of the body, and *minus* pressure or partial vacuum throughout the tail end. And as it seemed that the number of particles which would have to be thus dealt with would depend on the area of maximum cross section of the body, or area of ship's way, as it was sometimes termed, the resistance was supposed to bear an essential proportion to the midship section of the ship. This idea has sometimes been emphatically embodied in the proposition that the work a ship has to do in performing a given voyage is to excavate in the surface of the sea, from port to port, a canal the cross section of which is the same as the midship section of the ship.

This theory of resistance was at first sight natural and reasonable; it was generally admitted for many years to be the only practicable theory, and was embodied in all the most approved text-books on hydraulics and naval architecture. But when the theory of stream-lines was brought to bear upon the question, then it was discovered that the reactions, which the inertia of the fluid would cause against the surface of the body moving through it, and which were supposed to constitute the resistance, arranged themselves in a totally different manner from what had previously been supposed, and that, therefore, the old way of estimating their total effect upon the ship was fundamentally wrong. How wrong, I can best tell you by stating that according to the theory of stream-lines, a submerged body, such as a fish, for example, moving at a steady speed through the assumed frictionless fluid, would experience no resistance at all. In fact, when once put into motion it would go on for ever without stopping.

The revelation, then, which was brought about by the application of the stream-line theory to the question, amounted to this, that the approved formulæ for estimating the resistance of bodies moving through water were not only wrong in detail, but that the supposed cause of resistance, with which alone they professed to be dealing, was in reality no cause at all; and that the real cause of resistance, whatever it might be, was entirely left out.

It is easy to imagine how fruitful, in false aims and false principles of nautical construction, would be the assignment of the resistance of ships to a supposed cause which has no existence at all. And the old theory, though now discarded by scientific men, has obtained such a hold on the minds of the general public, that I hope you will excuse my devoting considerable space to its refutation.

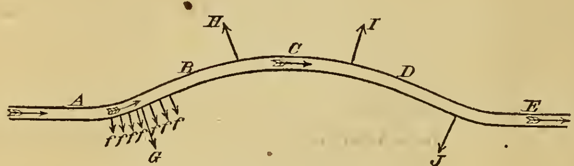
I will now briefly sketch an elementary view of the stream-line theory so far as it is relevant to our present purpose. Let it be understood that I am still dealing only with the supposed frictionless fluid; that for reasons which will hereafter appear, I am dealing not with a ship at the surface, but with a submerged body; and that I am supposing it to be traveling at a steady speed in a straight line. I am going to prove to you that under these circumstances the inertia of the fluid which has to be set in motion to make way for the body will cause no resistance to it. Not that such inertia will cause no pressures and suctions acting upon the surface of the body; far from it; but that the pressures and suctions so caused must necessarily so arrange themselves that the backward forces caused to the body on some parts of its surface will be neutralized by the forward forces caused on other parts. In effect, although the inertia of the fluid resists certain portions of the body, it propels the other portions of the body with a precisely equal force.

In showing how this comes about, I prefer to substitute for the submerged body moving through a stationary ocean of fluid the plainly equivalent conception of a stationary submerged body surrounded by a moving ocean of fluid. The proposition that such a body will experience no total endways push from the fluid flowing past it arises from a general principle of fluid motion, which I shall presently put before you in detail, namely, that to cause a frictionless fluid to change its condition of flow in any manner whatever, and ultimately to return to its original condition of flow, does not require, nay, does not admit of, the expenditure of any power; whether the fluid be caused to flow in a curved path, as it must do in order to get round a stationary body which stands in its way, or to flow with altered speed, as it must do in order to get through the local contraction of channel which the presence of the stationary body practically creates. Power, it may indeed be said, is being expended, and force exerted to communicate certain motions to the fluid; but that same power is also being given back, and the force counterbalanced, where the fluid is yielding up the motion which has been communicated to it, and is returning to its original condition.

In commencement I will illustrate these two actions by considering the behavior of fluid flowing through variously-shaped pipes; and I will begin with a very simple instance, which I will treat in some detail, and which will serve to show the nature of the argument I am about to submit to you.

Suppose a rigid pipe of uniform sectional area, of the form shown in Fig. 3, something like the form of the water-line of a vessel.

Fig. 3.



The portions A B, B C, C D, DE are supposed to be equal in length and of the same curvature, the pipe terminating at E in exactly the same straight line in which it commenced at A, so that its figure is perfectly symmetric on either side of C, the middle point of its length.

Let us now assume that the pipe has a stream of frictionless fluid running through it from A towards E, and that the pipe is free to move bodily endways.

It is not unnatural to assume at first sight that the tendency of the fluid would be to push the pipe forward, in virtue of the opposing surfaces offered by the bends in it—that both the divergence between A and C from the original line at A, and the return between C and E to that line at E, would place parts of the interior surface of the pipe in some manner in opposition to the stream or flow, and that the flow thus obstructed would drive the pipe forward; if, however, we endeavor to build up these supposed causes in detail we shall find the reasoning to be illusory, and I will now trace the results which can be established by correct reasoning.

The surface being assumed to be smooth, the fluid, being a frictionless fluid, can exercise no drag by friction on the side of the pipe in the direction of its length, and in fact can exercise no force on the side of the pipe, except at right angles to it. Now the fluid flowing round the curve from A to B will, no doubt, have to be deflected from its course, and its inertia, by what is commonly known as centrifugal action, will cause pressure against the outer side of the curve, and this with a determinable force. The magnitude and direction of this force at each portion of the curve of the pipe between A and B are represented by the small arrows marked f; and the aggregate of these forces between

A and B is represented by the larger arrow marked G. In the same way the forces acting on the parts B C, C D, and D E are indicated by the arrows H, I, and J; and as the conditions under which the fluid passes along each of the successive parts of the pipe are precisely alike, it follows that the four forces are exactly equal, and, as shown by the arrows in the diagram, they exactly neutralize one another in virtue of their respective directions; and therefore the whole pipe from A to E, considered as a rigid single structure, is subject to no disturbing force by reason of the fluid running through it.

Though this conclusion that the pipe is not pushed endways may appear on reflection so obvious as to have scarcely needed proof, I hope that it has not seemed needless, even though tedious, to follow somewhat in detail the forces that act, and which, under the assumed conditions, are the only forces that act, on a symmetrical pipe such as I have supposed.

Having shown that in the instance of this special symmetrically-curved pipe the flow of a frictionless fluid through it does not tend to push it endways, I will now proceed to show that this is also the case whatever may be the outline of the pipe, provided that its beginning and end are in the same straight line.

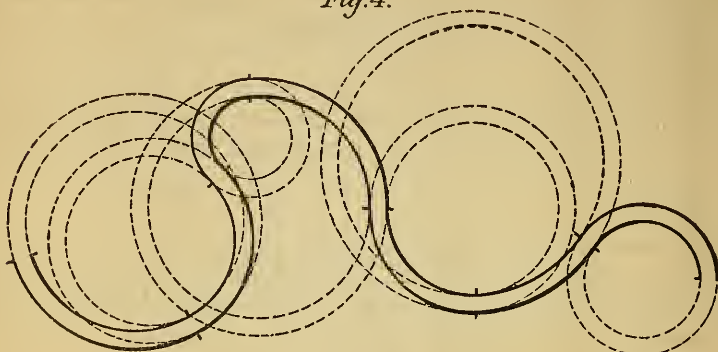
Assume a pipe bent into a complete circular ring with its ends joined, and the fluid within it running with velocity round the circle. The inertia of this fluid, by centrifugal force, exercises a uniform outward pressure on every part of the uniform curve; and this is the only force the fluid can exert. This outward pressure tends to enlarge or stretch the ring, and thus causes a uniform circumferential tension on each side of the ring.

Now take a ring of twice the diameter and suppose the fluid to be running around it with the same linear velocity as before. The diameter of the curve being doubled, and the speed being the same, the outward pressure due to centrifugal force on each linear inch of the ring will be halved; but since the diameter is doubled, the number of linear inches in the circumference of the ring will be doubled. Since, then, we have twice the number of inches acting, each with half the force, the total force tending to enlarge the ring will be unaltered, and the circumferential tension on the ring, caused by the centrifugal force of the fluid, will be just the same as before.

In the same way we can prove that in any number of rings of any diameters, if the linear velocity of the fluid in each is the same, the circumferential tension caused by the centrifugal force of the fluid will also be the same in each.

Now let us take each of these rings and cut out a piece, and then join all these pieces together so as to form a continuous pipe, as in Fig. 4, and suppose the stream of fluid flowing through the combined pipe with the same linear velocity as that with which it was before flowing round each of the rings. The fluid in each of the segments will now be in precisely the same condition as when the segment formed part of a complete ring, and will subject each piece of ring to the same strains as before, namely, to a longitudinal tension or strain, and to that only. And since we have already seen that the tension is the same in amount in each ring, the tension will be the same at every point in the combined pipe.

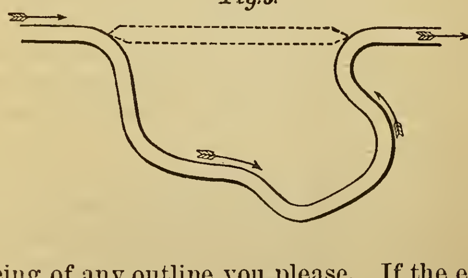
Fig. 4.



This being so, if we imagine the pipe to be flexible (but not elastic), and to be fastened at the ends, the pipe, although flexible, will not tend to be disturbed in its shape by the inertia of the fluid which is running through it; because the fluid does not cause any lateral force, but only a longitudinal stretching force, and that the same in amount at every point. And this will clearly be so in a pipe of any outline, because any curve may be made up by thus piecing together short bits of circular arcs of appropriate radii.

Let us then take a flexible pipe having the two ends in the same straight line but pointing away from one another as in Fig. 5, the inter-

Fig. 5.



mediate part being of any outline you please. If the ends are fixed we have seen that the flow of fluid will not tend to disturb the pipe, and therefore all that will be necessary to hold it in its position will be an equal and opposite tension supplied by the anchorages at the ends, to prevent the ends being forced towards one another. And if, instead of anchoring the ends, we put a strut between them to keep them apart, the pipe thus fitted will require no external force to keep it in position. In other words, whatever be the outline of a pipe, provided its beginning and end are in the same straight line, a frictionless fluid flowing through it will have no tendency to push it bodily endways.

So far I have dealt only with pipes having uniform sectional area throughout their length, an assumption which has been necessary to the treatment pursued, as the velocity has in each case been assumed to be uniform throughout the length of the pipe. I will now proceed to consider the behavior of fluid flowing through pipes of varying sectional area and consequently flowing with varying velocity.

It is, I think, a very common impression that a fluid in a pipe, meeting a contraction of diameter (see Fig. 6), exercises an excess of press-

Fig. 6.

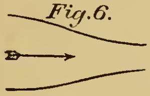
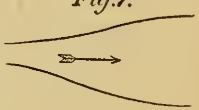
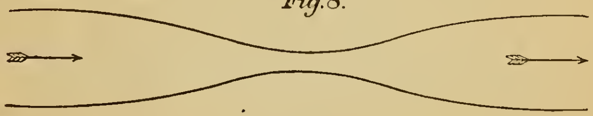


Fig. 7.



ure against the entire converging surface which it meets, and that conversely, as it enters an enlargement (see Fig. 7), a relief of pressure is experienced by the entire diverging surface of the pipe. Further, it is commonly thought that there is in the narrow neck of a contracted passage (see Fig. 8) an excess of pressure due to the squeezing together of the fluid at that point.

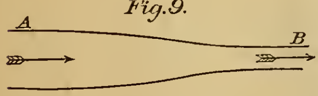
Fig. 8.



These impressions are in every respect erroneous; the pressure at the smallest part of the pipe is, in fact, less than that at any other point, and *vice versa*.

If a fluid be flowing along a pipe, A B, which has a contraction in it (see Fig. 9), the forward velocity of the fluid at B must be greater than that at A, in the proportion in which the sectional area of the pipe at B is less than that at A; and, therefore, while passing from A to B the forward velocity of the fluid is being increased.

Fig. 9.



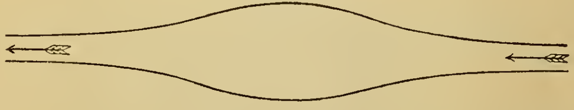
This increase of velocity implies the existence of a force acting in the direction of the motion, to overcome the inertia of the fluid; that is to say, each particle which is receiving an increase of forward velocity must have a greater fluid pressure behind it than in front of it; for no other condition will cause that increase of forward velocity. Hence a particle of fluid, at each stage of its progress along the tapering contraction, is passing from a region of higher pressure to a region of lower pressure, so that there must be a greater pressure in the larger part of the pipe than in the smaller, the diminution of pressure at each point corresponding with the diminution of sectional area, corresponding, that is to say, with the additional forward velocity assumed by the fluid at each point of its advance along the contraction. Consequently, differences of pressure at different points in the pipe depend solely upon the velocities, or, in other words, on the relative sectional areas of the pipe, at those points.

It is easy to apply the same line of reasoning to the converse case of an enlargement. Here the velocity of the particles is being reduced through precisely the same series of changes, but in an opposite order. The fluid in the larger part of the pipe moves more slowly than that in the smaller, so that, as it advances along the enlargement, its forward velocity is being checked; and this check implies the existence of a force acting in a direction opposite to the motion of the fluid, so that each particle which is being thus retarded, must have a greater fluid pressure in front of it than behind it; thus a particle of fluid at each stage of its progress along a tapering enlargement of a pipe is passing from a region of lower pressure to a region of higher pressure, the change of pressure corresponding to the change of velocity required. Hence we see that a given change of sectional area will require the same change of pressure, whether the pipe be an enlargement or a contraction.

Therefore, in a pipe in which there is a contraction and a subsequent enlargement to the same diameter as before (see Fig. 8), since the differences of pressure at different points depend on the differences of sectional area at those points, by a law which is exactly the same in an enlarging as in a contracting pipe, the points which have the same sectional areas will have the same pressures, the pressures at the larger areas being larger, and those at the smaller areas smaller.

Precisely the same result will follow in the case of an enlargement followed by a contraction (see Fig. 10).

Fig. 10.



Were water a frictionless fluid these propositions could be exactly verified by experiment as follows:

Figs. 11 and 12 show certain pipes, the one a contraction followed by an enlargement, the other an enlargement followed by a contraction. At certain points in each pipe there are small holes, communicating with vertical gauge-glasses. The height at which the fluid stands in each of these vertical glasses, of course indicates the pressure in the pipe at the point of attachment.

In Fig. 11 the sectional areas at E and P are equal to one another, Those at C and K are likewise equal to one another, but are smaller than those at E and P. The area at I is the smallest of all. Now, the fluid being frictionless, the pressures at E and P indicated by the heights E D and P Q would be equal, these being greater than C H and K N. C H and K N would also be equal to one another, and would be themselves greater than I J.

Fig. 11.

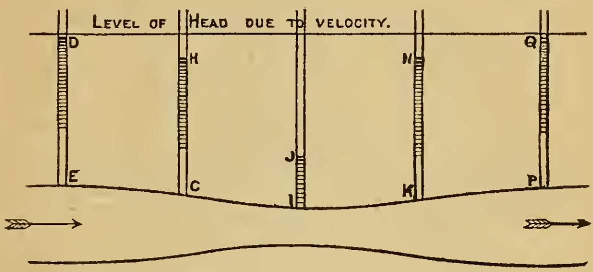
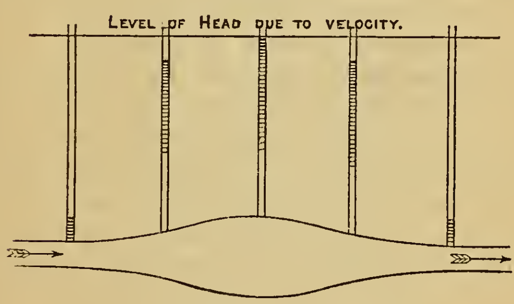
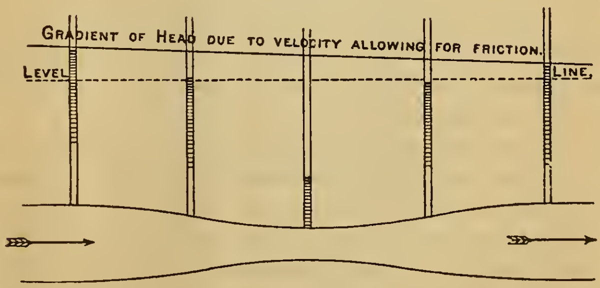


Fig. 12.



The results shown in Fig. 12 are similar in kind, equal pressures corresponding to equal sectional areas.

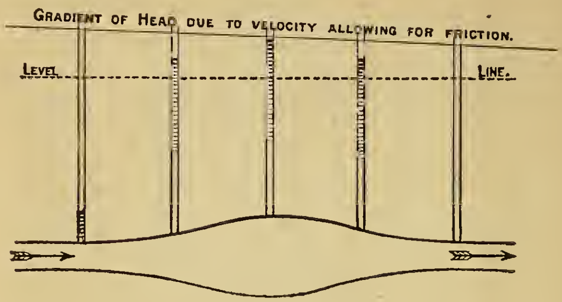
Fig. 13.



But if the experiment were tried with water, some of the pressure at each successive point would be lost in friction, and this growing defect in pressure, or "gradient," would be indicated in the successive gauge-glasses in the manner shown in Figs. 13 and 14.

I have here arranged an experiment which conveniently illustrates these propositions, making allowance for the frictional gradient.

Fig. 14.



h k l e f g a b c (see Fig. 15) is a continuous series of glass tubes, through which water is flowing from the cistern *n* to the outlet *m*. The cistern is kept full to a certain level. The tube from *h* to *l* is what I have called an enlargement followed by a contraction (like Fig. 10); from *c* to *g*, the diameter is the same throughout; and from *a* to *b*, the tube is a contraction followed by an enlargement (like Fig. 8). Just as in Figs. 11, 12, 13, 14, gauge-glasses are here fitted to the various tubes, to show the pressures of the water in them at various points.

Let us first consider the parallel pipe *e g*. If the fluid were frictionless, the diameter being uniform, the pressure would be uniform throughout, and the fluid would stand at the same level in each of the three gauge-glasses. But, owing to the friction, the water surfaces in the three glasses do not come up to a level line, but form a descending line, namely, the frictional gradient.

Now take the pipe *a c*. Here the smallest pressure denoted by the water level at *b′*, is in the middle at *b*, where the diameter is smallest, and the greatest pressure denoted by the water levels at *a′*, *c′*, is at the two ends *a c*, where the diameter is greatest. And if the fluid were frictionless, the pressure at the two ends, which have the same diameter, would be the same, but with water there is, as in the parallel pipe *c g*, a gradient or loss of pressure due to the friction.

The frictional gradient, according to well-known hydraulic rules, has a definite law of variation in terms of diameter and velocity, consequently it has been possible by calculation to so arrange the diameters of the pipes that the parallel pipe *e g* should, according to the rule, have the same frictional gradient as the pipe *a c*, and as we see that the gradients are in fact the same, the result not merely illustrates but verifies the propositions.

In the pipe *h k l* we have the smallest diameter at the two ends *h* and *l*, and the largest diameter at the middle point *k*, and consequently we have the smallest pressures denoted by the water levels at *h′* and *l′*,

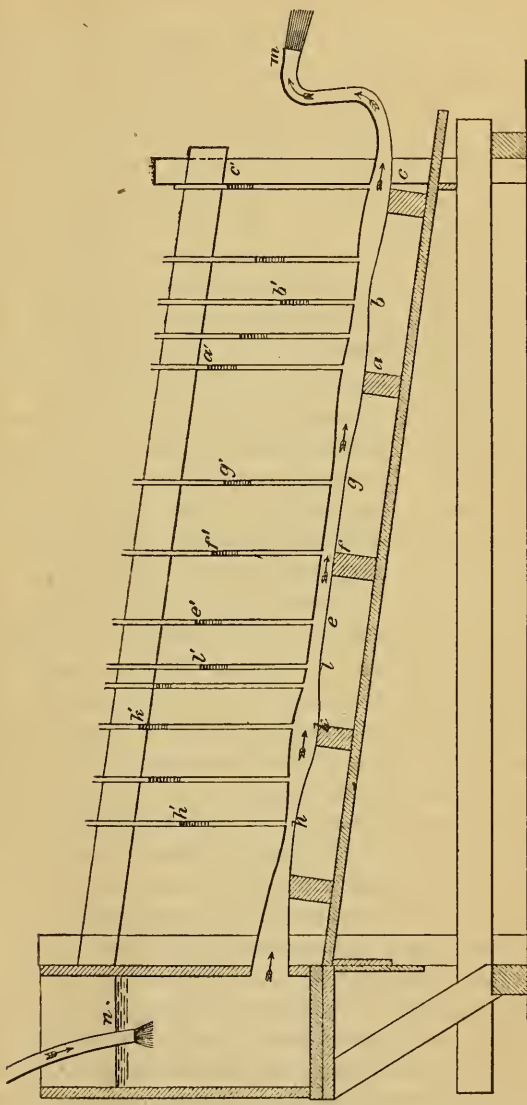
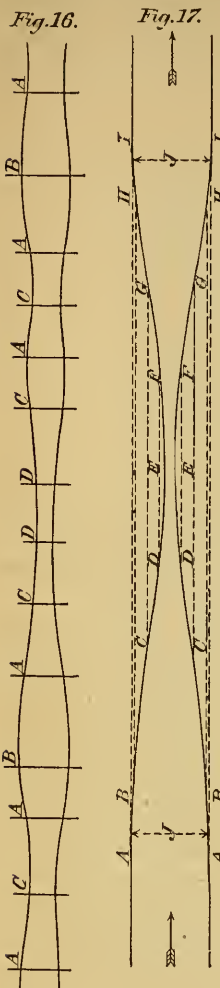


Fig. 15.

at the two ends, and the greatest pressure in the middle denoted by the water level at *k′*, and we again have the fall or gradient from end to end due to friction.

These experiments afford a good verification of the proposition which

I just now explained, namely, that in a frictionless fluid flowing through a pipe of varying diameter, the pressure at each point depends on the sectional area at that point, there being equal pressures at the points of equal sectional area. Hence if in the pipe shown in Fig. 16 the areas at all the points marked A are equal, if also the areas at all the points marked B are equal, and so also with those at C and D, then the pressures at all the points A will be the same, the pressures at all the points B will be the same, and so with those at C and D.



Since, then, the pressure at each point depends on the sectional area at that point and on that only it is easy to show that the variations in pressure due to the flow are not such as can cause any total endways force on the pipe, provided its sectional area at each end is the same.

Take for instance the pipe shown in Fig. 17. The conical portion of pipe A B presents the same area of surface effective for endways pressure as does the conical portion H I, only in opposite directions. They are both subject to the same pressure, being that appropriate to their effective mean diameter J. Consequently the endways pressures on these portions are equal and opposite, and neutralize one another. Precisely in the same way it may be seen that the endways pressures on B C, C D, D E, exactly counteract those on G H, F G, E F; and it may be similarly shown, that in any combination whatever of enlargements and contractions; provided the sectional area and direction of the pipe at the two ends are the same, the total endways force impressed on the pipe by the fluid flowing through it must be *nil*.

We see then that a frictionless fluid flowing through a pipe of any form, whether tortuous or of varying diameter, will not tend to push it endways, as long as the two ends of the pipe are in the same straight line, and have the same sectional area; in a word, as long as the speed and direction of flow of the fluid are the same in leaving the pipe as in entering it; and in this compound proposition concerning the flow of fluid through pipes, I have laid the necessary foundation for the treatment of the case of the flow of an ocean of frictionless fluid past a submerged body.

I have dealt with the instance of a single stream of uniform sectional area (and therefore of uniform velocity of flow), enclosed in a pipe of any outline whatever, and I have dealt with the instance of a single stream of varying sectional area and velocity of flow; and in both these cases I have shown that, provided the streams or pipe contents finally return to their original direction and velocity of flow, they administer no total endways force to the pipe or channel which causes their deviations.

I am now going to deal with a combination of such streams, each to some extent curved and to some extent varying in sectional area, which, when taken together, constitute an ocean of fluid, flowing steadily past a stationary submerged body (see Fig. 18); and here also, since the combination of curved streams surrounding the body, which together constitute the ocean flowing past it, return finally to their original direction and velocity, they can not administer to the body any endways force. Every particle of the fluid composing this ocean, as it passes the body, must undoubtedly follow some path or other, though we may not be able to find out what path; and every particle so passing is preceded and followed by a continuous stream of particles all following the same path, whatever that may be. We may then, in imagination, divide the ocean into streams of any size and of any cross section we please, provided they fit into one another so as to occupy the whole space, and provided the boundaries which separate the streams exactly follow the natural courses of the particles.

If we trace the streams to a sufficient distance ahead of the body, we shall there find the ocean flowing steadily on, completely undisturbed by, and, so to speak, ignorant of, the existence of the body which it will ultimately have to pass. There, all the streams must have the same direction, the same velocity of flow, and the same pressure. Again, if we pursue their course backwards to a sufficient distance behind the body, we shall find them all again flowing in their original direction; they will also have all resumed their original velocity; for otherwise, since the velocity of the ocean as a whole can not have changed, we should have a number of straight and parallel streams having different velocities side by side with one another. This, in a frictionless fluid, would be clearly an impossible state of things, for we have seen that in a frictionless fluid the velocities exactly correspond with the pressures, so that if the velocities of these streams were different the pressures would be different, and if the pressures were different the fluid would begin to flow from the greater pressures towards the less, and the streams would thus become curved instead of straight.

Thus, although in order to get past the body these streams follow some courses or other, various both in direction and velocity, settling themselves into these courses in virtue of the various reactions which they exert upon one another and upon the surface of the body, yet ultimately, and through the reverse operation of corresponding forces, they settle themselves into their original direction and original velocity. Now, the sole cause of the original departure of each and all of these streams from, and of their ultimate return to, their original direction and velocity, is the submerged stationary body; consequently the body must receive the sum total of the forces necessary to thus affect the streams. Conversely this sum total of force is the only force which the passage of the fluid is capable of administering to the body. But we know that to cause a single stream, and therefore also to cause any combination or system of streams, to follow any courses, changing at various points both in direction and velocity, requires the application of forces the sum total of which in a longitudinal direction is *nil*, provided that the end of each stream has the same direction and velocity as the beginning. Therefore the sum total of the forces (in other words the only force) brought to bear upon the body by the motion of the fluid in the direction of its flow is *nil*.

Another instructive way of regarding the same problem is this: Suppose each and every one of the streams into which we have subdivided the ocean to be inclosed in an imaginary rigid pipe made exactly to fit it throughout, the skin of each pipe having no thickness whatever. The innermost skin of the innermost layer of pipes (I mean that layer which is in contact with the side of the body), the innermost skin, I say, of this layer is practically neither more nor less than the skin or surface of the body. The other parts of the skins of this layer, and all the skins of all the other pipes, simply separate fluid from fluid, which fluid *ex hypothesi* would be flowing exactly as it does flow if the skins of the pipes were not there; so that, in fact, if the skins were perforated, the fluid would nowhere tend to flow through the holes. Under these circumstances the flow of the fluid clearly can not bring any force to bear on any of the skins of any of the pipes, except on the innermost skin of the innermost layer. Now, we know that the fluid flowing through this system of pipes administers no total endways force to any one of the pipes or to the system as a whole; but it produces, as we have just seen, no force whatever upon any of the skins which separate fluid from fluid; consequently, if these are removed altogether, the force administered to the remainder of the system will be the same as is administered to the whole system, namely, no total endways force whatever. But what is this remainder of the system which has no total endways force upon it? Simply the surface of the body, which is formed, as I have already said, by the innermost skins of the innermost layer of pipes. Therefore no total endways force is administered to the body by the flow of the fluid.

I have now shown that an infinite ocean of frictionless fluid flowing past a stationary submerged body can not administer to it any endways force, whatever be the nature of the consequent deviations of the streams of fluid. The question, what will be in any given case the precise configuration of those deviations, is irrelevant to the proof I have given of this proposition. Nevertheless it is interesting to know something at least of the general character which these deviations, or "stream-lines," assume in simple cases; therefore I show some in Figs. 18 and 19, which are drawn according to the method explained by the late Professor Rankine.

The longitudinal lines represent paths along which particles flow; they may therefore be regarded as boundaries of the streams into which we imagined the ocean to be divided.

We see that, as the streams approach the body, their first act is to broaden, and consequently to lose velocity, and therefore, as we know, to increase in pressure. Presently they begin to narrow, and therefore quicken, and diminish in pressure, until they pass the middle of the body, by which time they have become narrower than in their original undisturbed condition, and consequently have a greater velocity and less pressure than the undisturbed fluid. After passing the middle they broaden again until they become broader than in their original condition, and therefore have less velocity and greater pressure than the undisturbed fluid. Finally, as they recede from the body, they narrow again until they ultimately resume their original dimension, velocity, and pressure. Thus, taking the pressure of the surrounding undisturbed fluid as a standard, we have an excess of pressure at both the head and stern ends of the body, and a defect of pressure along the middle.

We proved just now that, taken as a whole, the pressures due to the inertia of the fluid could exert no endways push upon the stationary body. We now see something of the way in which the separate pressures act, and that they do not, as seems at first sight natural to expect, tend all in the direction in which the fluid is flowing; on the contrary, pressure is opposed to pressure, and suction to suction, and the forces neutralize one another and come to nothing; and thus it is that an ocean of frictionless fluid, flowing at steady speed past a stationary submerged body, does not tend to push it in the direction of the flow. This being so, a submerged body traveling at a steady speed through a stationary ocean of frictionless fluid will experience no resistance.

Since then a frictionless fluid would offer no resistance to a submerged body moving through it, we have next to consider what are the real causes of the resistance which such a body experiences when moving through water.

The difference between the behavior of water and that of the frictionless fluid is twofold, as follows:

First. The particles of water, unlike those of a frictionless fluid, exert a drag or frictional resistance upon the surface of the body as they glide along it. This action is commonly called surface-friction or skin-friction, and its amount in any given case can be calculated from general experimental data. The resistance due to the surface-friction of a body such as that which we have been considering is practically the same as that of a plane surface of the same length and area, moving at the same speed edgewise through the water.

The second difference between the behavior of water and that of the imaginary frictionless fluid surrounding the moving submerged body is that the mutual frictional resistance experienced by the particles of water in moving past one another somewhat hinders the necessary stream-line motions, alters their nice adjustment of pressures and velocities, defeats the balance of forward and backward forces acting

Fig. 18.

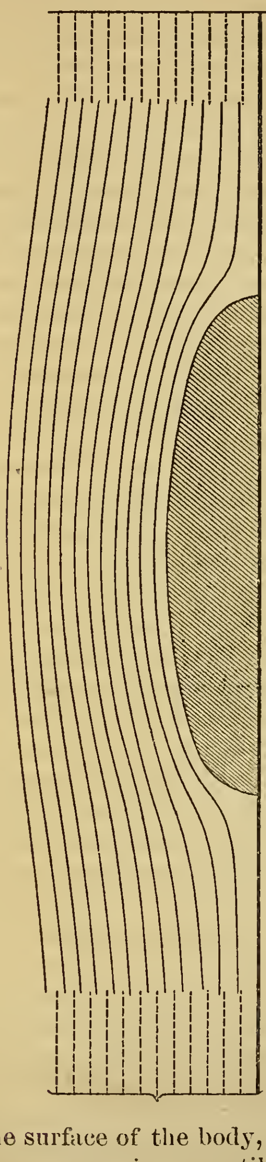
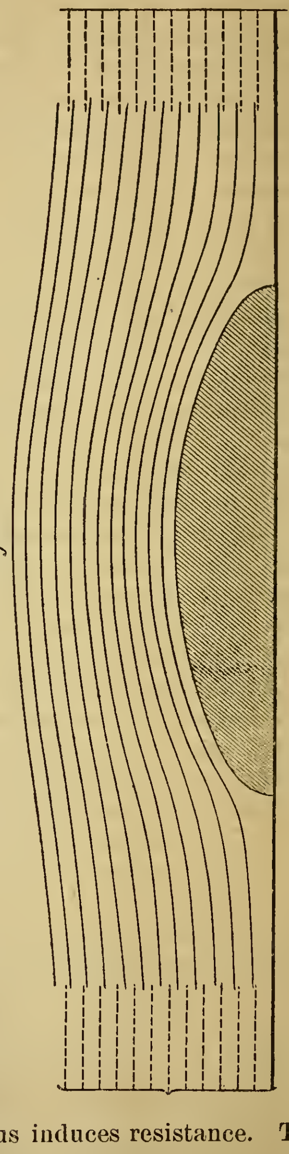


Fig. 19.



against the surface of the body, and thus induces resistance. This action, however, seems imperceptible in forms of fairly easy shape, such as that shown in Fig. 2, and only operates tangibly where there are angular features, or very blunt sterns, like the blunt round tail, for instance, of the bodies shown in Figs. 18 and 19. In such a case, the stream-lines, instead of closing in round the stern, as shown in the figures, form a swirl or eddy, from which it results that the excess of pressure which would exist at the tail end in a frictionless fluid, and which would there counterbalance the similar excess of pressure at the nose of the body, becomes in water greatly reduced, and in part converted into negative pressure, and thus a very great resistance may result. It is worth mentioning, however, that it is blunt tails rather than blunt noses that cause these eddies, and thus a body with one end round and the other sharp no doubt experiences least resistance when going with the round end first.

I call this course of resistance "eddy-making resistance," and, as I have said, it will be imperceptible in forms of fairly easy shape, such, for example, as Fig. 2. Such a form of submerged body will experience practically no resistance except that due to surface-friction, and will therefore experience practically only the same total resistance as a thin plane, like Fig. 1, moving edgewise, which possesses the same area of wetted skin. In fact, we may say generally that all submerged bodies of fairly fine lines experience no resistance except surface-friction.

I have hitherto, throughout the whole of this reasoning, been dealing with submerged bodies only, by which I mean bodies traveling at a great depth below the surface of the fluid; and I have shown the sole causes of their resistance to be the two I have termed, respectively, surface-friction and eddy-making resistance. But when we come to the case of a ship, or any other body traveling at or indeed near the surface, we find a new cause of resistance introduced; a cause the consideration of which is often of most vital importance in the design of the forms of ships, and which renders the question of the form of least resistance for a ship entirely different from that of the form of least resistance for a submerged body. This new cause of resistance, like the eddy-making resistance, operates by altering the stream-line motions and defeating their balance of forward and backward forces. It arises as follows:

Imagine a ship traveling at the surface of the water, and first let us suppose the surface of the water to be covered with a sheet of rigid ice, and the ship cut off level with her water-line, so as to travel beneath the ice, floating, however, exactly in the same position as before. (See Fig. 20.) As the ship travels along the stream-line motions will be the

Fig. 20.

same as for a submerged body, of which the ship may be regarded as the lower half; and the ship will move without resistance, except that due to the two causes I have just spoken of, namely surface-friction and eddy-making resistance. The stream-line motions being the same in character as those we have been considering, we shall still have at each end an excess of pressure and along the sides a defect of pressure, which will tend the one to force up the sheet of ice and the other to suck it down. If now we remove the ice, the water will obviously rise in level at each end, in order that excess of hydrostatic head may afford the necessary reaction against the excess of pressure, and the water will sink by the sides, in order that defect of hydrostatic head may afford reaction against the defect of pressure.

The hills and valleys which thus commence to be formed in the water are, in a sense, waves, and though originating in the stream-line forces of the body, yet, when originated, they come under the dominion of the ordinary laws of wave motion, and to a large extent behave as independent waves; and in virtue of their independent action they modify the stream-line forces which originated them, and alter the pressures which are acting upon the surface of the ship.

The exact nature of this alteration of pressure, in any given case, we have no means of predicting; but we can be quite sure it must operate to alter the balance of forward and backward forces in such a way as to cause resistance; for we see that the final upshot of all the different actions which take place is this—that the ship in its passage along the surface of the water has to be continually supplying the waste of an attendant system of waves, which, from the nature of their constitution as independent waves, are continually diffusing and transmitting themselves into the surrounding water, or, where they form what is called broken water, crumbling away into froth. Now, waves represent energy, or work done, and therefore all the energy represented by the waves wasted from the system attending the ship is so much work done by the propellers or tow-ropes which are urging the ship. So much wave-energy wasted per mile of travel is so much work done per mile, and so much work done per mile is so much resistance.

The surface of the water thus admits of an escape, as it were, of the pressures which arise from the inertia of the particles of the fluid which have to be set in motion by the body. But so far from thereby rendering less obstruction to the passage of the body, these pressures are enabled by that very escape to result in a resistance, which, if they were confined by the fluid overhead, as with a submerged body, they would have been unable to produce; in fact, at the surface the particles are able to escape the duty of restoring to the body the power which the body employed to set them in motion. There can be no doubt that in this way a fish, when swimming so close to the surface as to make waves, experiences more resistance than when deeply immersed.

It is worth remark that this cause of resistance, "wave-genesis" or "wave-making resistance," as it has been termed, would be equally a cause of resistance in a frictionless fluid, and it is for this reason that in proving to you just now that a body would experience no resistance in moving through a frictionless fluid, I limited the case to that of a submerged body. It is true that in a frictionless fluid the wave system generated by a ship would not waste away, as in water, by its internal friction; but it would none the less be diffused into the surrounding fluid, and thus, as the ship proceeded, she would cover a larger and larger area of ocean surface with the waves she was making.

Having arrived at this point, I think it will be useful briefly to review the several cases of motion through fluid, in order to trace where the several causes of resistance we have dealt with come into operation.

Case I.—A plane moving edgeways through frictionless fluid. Here there will be no resistance.

Case II.—A plane moving edgeways through frictional fluid. Here there will be resistance due to surface friction.

Case III.—A submerged body moving through frictionless fluid. The inertia of the fluid undergoing stream-line motion causes excess of pressure at the two ends and defect of pressure along the middle. The forward and backward pressures balance one another, and therefore cause no resistance.

Case IV.—A submerged body moving through frictional fluid. Here there is resistance due to surface friction. Also, if the body is abrupt enough to cause eddies, part of the excess of pressure at the tail-end will be converted by the friction of the particles of fluid into defect of pressure, and so will destroy the balance between the forward and backward pressures, thus causing eddy-making resistance.

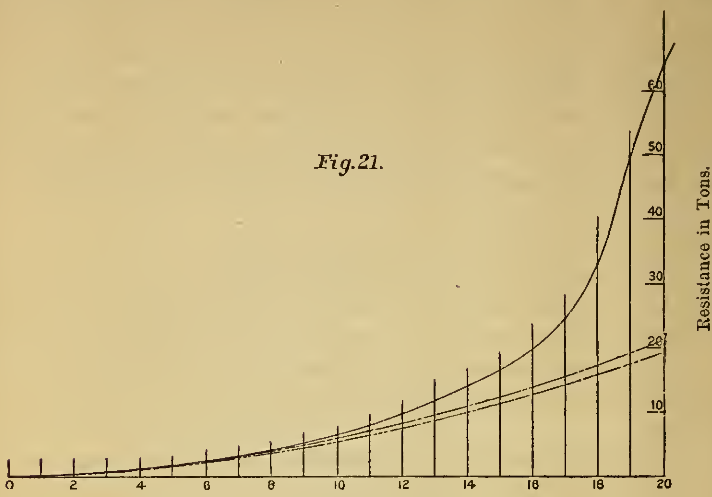
Case V.—A body moving through frictionless fluid, but at or near the surface. The direct pressures on the surface of the body are altered by the operation of the wave system which has been created, thus destroying the balance of forward and backward forces, and introducing "wave-making resistance."

Case VI.—A body moving through frictional fluid at or near the surface. Here surface-friction, eddy-making resistance, and wave-making resistance will act in combination, and will together make up the total resistance.

Having thus reviewed the several operations which will combine to cause resistance to a ship moving at the surface of the water, it will be interesting to see in what proportion they are combined in an actual ship of ordinary form; and, to take a single instance, I show the "curves of resistance," as they are called, of the S. S. *Merkara*, a mercantile ocean steam-ship of 3,980 tons. It is perhaps necessary to explain that a curve of resistance is a diagram constructed to show at a glance the resistance at any speed, so that if any point be taken on the scale of speed, forming the base-line, the ordinate or vertical height from the point to the curve above, measured by the scale of force, will show the amount of resistance at that speed. Thus, in Fig. 21, where the uppermost line represents the total resistance of the ship, we see that at a speed of 12 knots the resistance, as indicated by the height up to the line, is 9.3 tons.

The plain line on Fig. 21 is the curve of total resistance of the *Merkara* deduced from experiments made with a model of that ship.

Fig. 21.



The lowest of the two dotted lines is the curve of surface-friction resistance of the ship, calculated from experiments made upon the resistance of thin planes moving edgeways through water. The space between the foregoing line and the dotted line immediately above it represents the amount of resistance due to eddy-making, deduced from data which it would take too long to describe here. The space between this upper dotted line and the plain line above it is the wave-making resistance.

We see, then, that with this ship the eddy-making resistance is about 8 per cent. of the surface-friction at all speeds. We see, further, that at 8 knots the wave-making resistance is practically *nil*, that at 11 knots it is only 12 per cent. of the whole resistance at that speed, and that at 13 knots, which is the maximum speed of the ship, it is 17 per cent. of the whole. As we go further up in the scale of speed the wave-making resistance mounts up very largely, and at 19 knots is fully 60 per cent. of the whole resistance.

The curve of resistance here given may be taken as a fair sample of those of ships of good build. It may be said generally that the eddy-making resistance is a comparatively small amount, and that it bears at all speeds nearly a constant proportion to the surface-friction. The wave-making resistance, on the contrary, always increases with increase of speed at a more rapid rate than the surface-friction, being generally *nil* at a very low speed, and becoming, at very high speeds, more than half of the whole resistance. Large ships, however, do not often attain under steam speeds at which the wave resistance is more than some 40 per cent. of the whole.

It is a point worth noticing here what an exceedingly small force, after all, is the resistance of a ship compared with the apparent magnitude of the phenomena involved. Scarcely any one, I imagine, seeing, for instance, the new frigate *Shah* steaming at full speed would be inclined at first sight to credit what is nevertheless the fact, that the whole propulsive force necessary to produce that apparently tremendous effect is only 27 tons; in fact, less than one two-hundredth part of the weight of the vessel. And of this small propulsive force at least 15 tons, or more than one-half, is employed in overcoming surface-friction simply.

Thus, although the vessel carries at her bow a wave 7 feet high, the forces which produce this are so far neutralized by other similar forces that the whole of her resistance, exclusive of surface-friction, might be represented by the sternward pressure on her bow which would be due to a single wave 14 inches high. Indeed, a wave 30 inches high would represent a sternward pressure equal to the whole resistance of the ship.

The truth is that the forces which are at work, namely, the stream-line pressures due to the inertia of the fluid, are indeed very great; what we have to deal with, in the shape of eddy-making or wave-making resistance, is nothing but a minute difference or defective balance between these great forces, and fortunate it is that they balance as well as they do. With a well-shaped ship at moderate speed we have scarcely any resistance but skin friction, for the balance of stream-line pressures is almost perfect; but, nevertheless, they are all the while in full operation, a forward force counteracting a backward force, each equal to perhaps five times the existing total resistance of the ship. We can easily imagine, then, that when we once begin to tamper with this balance we may produce unexpectedly great resistance; and thus when we are dealing with speeds at which the wave-making resistance comes into play, a small variation in form may cause a comparatively large variation in the wave-making resistance. It is this fact which gives the wave-making resistance such a vital importance in connection with the designing of ships; but, unfortunately, although the surface-friction element of resistance is easily calculated in all cases from general experimental data, neither theory nor general experiment have as yet supplied means of calculation applicable to the wave-making resistance. In the absence of this knowledge we have to rely on direct experiments with different forms of vessels, and to supply these is one of the objects of the experiments upon the resistances of models of various forms which I am now conducting for the Admiralty.

By these experiments I hope not only to obtain a great many comparisons, showing at once the superiority of some forms over others, but to deduce general laws by which the influence of variation of form upon wave-making resistance may be predicted. Already, indeed, some most instructive propositions concerning the operations of this cause of resistance have shaped themselves; but it would take far too long to describe them in this discourse. I will merely refer to one broad principle which underlies most of the important peculiarities of the wave-making element of resistance.

We have seen that the waves originate in the local differences of pressure caused in the surrounding water by the vessel passing through it. Let us suppose, then, that the features of a particular form are such that these differences of pressure tend to produce a variation in the water level shaped just like a natural wave, or like portions of a natural wave, of a certain length.

Now, an ocean wave of a certain length has a certain appropriate speed, at which only it naturally travels, just as a pendulum of a certain length has a certain appropriate period of swing natural to it. And just as a small force recurring at intervals corresponding to the natural period of swing of a pendulum will sustain a very large oscillation, so, when a ship is traveling at the speed naturally appropriate to the waves which its features tend to form, the stream-line forces will sustain a very large wave. The result of this phenomenon is that, as a ship approaches this speed, the waves become of exaggerated size and run away with a proportionately exaggerated amount of power, causing corresponding resistance. This is the cause of that very disproportionate increase of resistance experienced with a small increase of speed when once a certain speed is reached, an instance of which is exhibited at a speed of about 18 knots in the curve of resistance shown in Fig. 21.

We thus see that the speed at which the rapid growth of resistance will commence is a speed somewhat less than that appropriate to the length of the wave which the ship tends to form. Now, the greater the length of a wave is the higher is the speed appropriate to it; therefore the greater the length of the waves which the ship tends to form the higher will be the speed at which the wave making resistance begins to become formidable. We may therefore accept it as an approximate principle that the longer are the features of a ship which tend to make waves the longer will be the waves which tend to be made, the higher will be the speed she will be able to go before she begins to experience great wave-making resistance, and the less will be her wave-making resistance at any given speed.

This principle is the explanation of the extreme importance of having at least a certain length of form in a ship intended to attain a certain speed; for it is necessary, in order to avoid great wave-making resistance, that the "wave features," as we may term them, should be long in comparison with the length of the wave which would naturally travel at the speed intended for the ship.

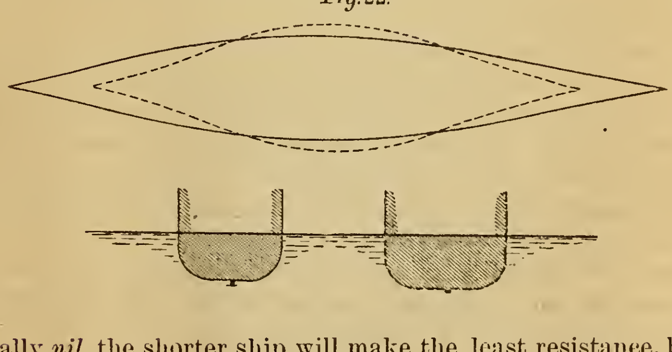
Time will not admit of my describing to you in detail how the principles I have been explaining affect the practical question of how to shape ships. I must leave you to imagine for yourselves, if you feel interested in following up the question, how the desirability of length of "wave features," for lessening wave resistance, is to a greater or less extent counteracted by the desirability of shortness of ship for lessening surface friction; and how in many other ways a certain variation of form, while it is a gain in one way is a loss in another, so that in every case the form of least resistance is a compromise between conflicting methods of improvement.

My principal object has been to combat the old fallacy of "head resistance," as it has been sometimes called, due to the inertia of the water acting against the area of the ship's way. I hope I have made it clear to you that the inertia of a frictionless fluid could offer no opposing force to a submerged body of any shape moving through it, for that the forces there developed by the inertia against the body must of necessity push it forward exactly as much as they push it backward, and that when the body is moving through a frictional fluid, or when it is moving at the surface of a fluid, this balance is only more or less destroyed through the operation of conditions which are totally independent of the area of midship section or area of ship's way.

For this reason, the only instances I have time to give you of the application of our knowledge of the causes of resistance to practical questions, shall be directly applicable as illustrations of the fallacy of the midship section theory.

Let us suppose that Fig. 22 represents the respective water lines of two vessels of the same tonnage but of different proportions of length to breadth. Now, it is true that the shorter of the two, when the speed of the wave appropriate to its wave features is approached, will experience great wave-making resistance, and will therefore probably experience greater total resistance than the longer ship. But it is certain that at low speeds, when the wave-making resistance of both ships is

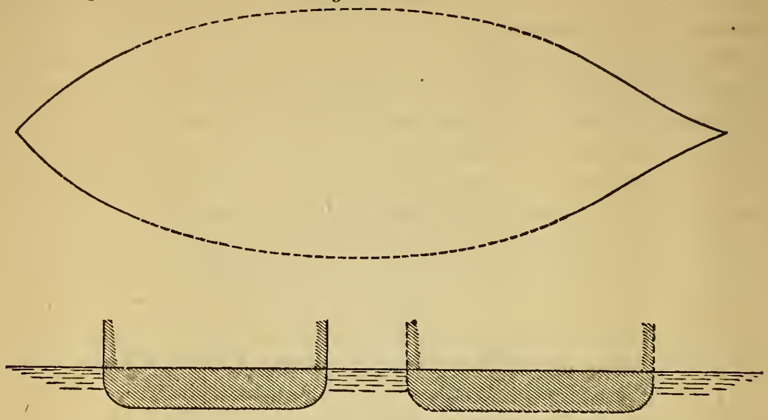
Fig. 22.



practically *nil*, the shorter ship will make the least resistance, because the long and narrow one has the largest area of skin, and will therefore have the greatest surface friction resistance. Judging, however, by the midship section theory, we should have erroneously concluded that the short and broad ship would make the greatest resistance of the two at all speeds.

Next let us take the two ships, whose water lines are shown in Fig. 23. It may be seen that the one shown in dotted lines has the same

Fig. 23.



length, and the same sharpness of ends as the other, but is filled out amidships to a larger cross-section. On the midship section theory this one would clearly have the greatest resistance of the two. Nevertheless, in the trial of two models of those lines it appeared that at the higher speeds the form with the largest cross-section made considerably the least resistance. The explanation of this lies, of course, in the fact that the addition amidships, though increasing the displacement, forms a prolongation of the wave features of the two ends, and thus lessens the wave-making resistance.

In conclusion, let me again insist, and with the greatest urgency, on the hopeless futility of any attempt to theorize on goodness of form in ships, except under the strong and entirely new light which the doctrine of stream-lines throws on it.

It is, I repeat, a simple fact that the whole framework of thought by which the search for improved forms is commonly directed consists of ideas which, if the doctrine of stream-lines is true, are absolutely delusive and misleading. And real improvements are not seldom attributed to the guidance of those very ideas which I am characterizing as delusive, while in realty those improvements are the fruit of painstaking but incorrectly rationalized experience.

I am but insisting on views which the highest mathematicians of the day have established irrefutably; and my work has been to appreciate and adapt these views when presented to me.*

* I can not pretend to frame a list of the many eminent mathematicians who originated or perfected the stream-line theory; but I must name from amongst them Professor Rankine, Sir William Thomson, and Professor Stokes, in order to express my personal indebtedness to them for information and explanations to which chiefly (however imperfectly utilized) I owe such elementary knowledge of the subject as alone I possess.

No one is more alive than myself to the plausibility of the unsound views against which I am contending; but it is for the very reason that they are so plausible that it is necessary to protest against them so earnestly; and I hope that in protesting thus I shall not be regarded as assuming too dogmatic a tone.

In truth, it is a protest of scepticism, not of dogmatism; for I do not profess to direct any one how to find his way straight to the form of least resistance. For the present we can but feel our way cautiously towards it by careful trials, using only the improved ideas which the stream-line theory supplies, as safeguards against attributing this or that result to irrelevant or rather non-existing causes.

4.

EXPERIMENTS UPON THE EFFECT PRODUCED ON THE WAVE-MAKING RESISTANCE OF SHIPS BY LENGTH OF PARALLEL MIDDLE BODY.

By W. Froude, Esq., M. A., F. R. S.

[Read at the eighteenth session of the Institution of Naval Architects, 23d March, 1877.]

I think I can best render what I have to say intelligible by giving a slight sketch of the system of experiment which I am carrying out for the Admiralty, and on certain of the results of which this paper is founded. That system of experiments involves the construction of models of various forms (they are really fair-sized boats of from 10 to 25 feet in length) and of testing by a dynamometer the resistances they experienced when running at various assigned appropriate speeds. The system may be described as that of determining the scale of resistance of a model of any given form, and from that the resistance of a ship of any given form, rather than as that of searching for the best form; and this method was preferred as the more general, and because the form which is best adapted to any given circumstances comes out incidentally from a comparison of the various results. We drive each model through the water at the successive assigned appropriate speeds by an extremely sensitive dynamometrical apparatus, which gives us in every case an accurate automatic record of the model's resistance, as well as a record of the speed. We thus obtain for each model a series of speeds, and the corresponding resistances; and to render these results as intelligible as possible, we represent them graphically in each case in a form which we call the "curve of the resistance" for the particular model. On a straight base line, which represents speed to scale, we mark off the series of points denoting the several speeds employed in the experiments, and at each of these points we plant an ordinate which represents to scale the corresponding resistance. Through the points defined by these ordinates we draw a fair curved line, and this curve constitutes what I have called the curve of resistance. This curve, whatever be its features, expresses for the model of that particular form what is in fact and apart from all theory the law of its resistance in terms of its speed; and what we have to do is if possible to find a rational interpretation of the law. Now we can at once carry the interpretation a considerable way; for we know that the model has so many square feet of skin in its surface, and we know by independent experiments how much force it takes to draw a square foot of such skin through the water at each individual speed. The law is very nearly—and for present convenience we may speak as if it were exactly—that skin resistance is as the area simply, and as the square of the speed. Now we have so many square feet of immersed skin in the model, and the total skin resistance is a certain known multiple of the product of that number of square feet, and of the square of the speed. Now, when we lay off on the curve of resistance a second curve which represents that essential and primary portion of the resistance, then we find this to be the result: The curve of skin resistance when drawn is found to be almost identical with the curve of total resistance at the lower speeds; but as the speed is increased, the curve of total resistance is found to ascend more or less, and in some cases to ascend very much above the curve of skin resistance. The identity of the two curves at the lower speeds is the practical representation of a proposition which the highest mathematicians have long been aware of, and which I have lately endeavored to draw the public attention to, and to render popularly intelligible; namely, that when a ship of tolerably fine lines is moving at a moderate speed, the whole resistance consists of surface friction. The old idea that the resistance of a ship consists essentially of the force employed in driving the water out of her way, and closing it up behind her, or, as it has sometimes been expressed, in excavating a channel through the track of water which she traverses—this old idea has ceased to be tenable as a real proposition, though *prima facie* we know that it was an extremely natural one. We now know that at small speeds practically the whole resistance consists of surface friction, and some derivative effects of surface friction, namely, the formation of frictional eddies, which is due to the thickness of the stem and of the sternpost; but this collateral form of frictional action is insignificant in its amount unless the features of the ship in which it originates are so abruptly shaped as to constitute a departure from that necessary fineness of lines which I have described; and we do not attempt to take an exact separate account of it. Thus we divide the forces represented by the curve of resistance into two elements—one "skin resistance" the other, which only comes into existence as the speed is increased, and which we may term "residuary resistance." And we have next to seek for the cause and governing laws of this latter element. Now, when the passage of the model along the surface of the water is carefully studied we observe that the special additional circumstance which becomes apparent as the speed is increased is the train of waves which she puts in motion; and indeed it has long been known that this circumstance has important bearings on the growth of resistance. It is in fact certain that the constant formation of a given series of waves involves the operation of a constant force and the expenditure of a definite amount of power, depending on the magnitude of those waves and the speed of the model; and as we thus naturally conclude that the excess of resistance beyond that due to the surface friction consists of the force employed in wave-making, we in a rough way call that residuary resistance "wave-making resistance." Perhaps I had better say a few words more about the nature and character of these waves. The inevitably widening form of the ship at her "entrance" throws off on each side a local, oblique wave of great or less size, according to the speed and to the obtuseness of the wedge, and these waves form themselves into a series of diverging crests, such as we are all familiar with. These waves have peculiar properties. They retain their identical size for a very great distance with but little reduction in magnitude. But the main point is that they become at once dissociated from the model, and after becoming fully formed at their bow they pass clear away into the distant water and produce no further effect on her resistance. But besides those diverging waves there is produced by the motion of the model another notable series of waves which carry their crests transversely to her line of motion. Those waves, when carefully observed, prove to have the figure shown in detail in Plate I. In that diagram there is shown the figure of a model which has a long parallel middle body accompanied by the series of these transverse waves as they appear at some one particular speed with the profile of the series defined against the side of the model; only I should mention that for the sake of distinctness the vertical scale of the waves has been made double the horizontal scale, so that they appear relatively to the model about twice as high as they really are. The profile is drawn from exact and careful measurements of the actual wave features as seen against the side of the model. It is seen that the wave is largest where its crest first appears at the bow, and it re-appears again and again as we proceed sternwards along the straight side of the model, but with successively reduced dimensions at each reappearance. That reduction arises thus: In proportion as each individual wave has been longer in existence its outer end has spread itself farther into the undisturbed water on either side, and as the total energy of the wave remains the same the local energy is less and less, and the wave crest, as viewed against the side of the ship, is constantly diminishing. We see the wave crest is almost at right angles to the ship, but the outer end is slightly deflected sternward from the circumstance that when a wave is entering undisturbed water its progress is a little retarded and it has to deflect itself into an oblique position, so that its oblique progress shall enable it exactly to keep pace with the ship. The whole wave-making resistance is the resistance expanded in generating, first, the diverging bow waves, which, as we have seen, cease to act on the ship when once they have rolled clear of the bow; secondly, these transverse waves, the crests of which remain in contact with the ship's side; and thirdly, the terminal wave, which appears independently at the stern of the ship. This latter wave arises from causes similar to those which create the bow wave, namely, the pressure of the streams, which forced into divergence then, here converge under the run of the vessel and re-establish an excess of pressure at their meeting. The term "wave-making resistance" represents then the excess of resistance beyond that due to surface friction, and that excess we know to be chiefly due to this formation of waves by the ship. With that explanation, I will proceed with my paper.

The experiments which form the subject of the present paper are a part of the series of systematic experiments on the resistances of models which I have been conducting. Their principal import, however, is to a great extent distinct from that of the rest of the series, and, as at the same time they throw valuable light upon the fundamental principles of resistance, I have thought it better to embody them in a separate paper.

The models used in the present experiments may be the best described as representing a series of imaginary ships of identical cross-section and identical forms of ends, the differences between them consisting in the length of parallel body (of uniform cross-section) inserted amidships. The lines of the longest ship of the series are shown in Fig. 1, the principal dimensions being as follows: Beam 38.4 feet, draught 14.4 feet, length of fore-body 80 feet, after-body 80 feet, parallel middle-body 340 feet, total length 500 feet. The other members of the series possess, of course, all the same dimensions, except the length of middle-body, which, in the several cases, is as follows: 340 feet (as above mentioned), 320 feet, 300 feet, 280 feet, 260 feet, 240 feet, 210 feet, 180 feet, 160 feet, 140 feet, 120 feet, 100 feet, 80 feet, 60 feet, 50 feet, 40 feet, 30 feet, 20 feet, 10 feet, 0. feet; the total lengths of ship being, consequently, 500 feet, 480 feet, 460 feet, 440 feet, 420 feet, 400 feet, 370 feet, 340 feet, 320 feet, 300 feet, 280 feet, 260 feet, 240 feet, 220 feet, 210 feet, 200 feet, 190 feet, 180 feet, 170 feet, and 160 feet.

The models of the ships from 500 to 280 feet long, inclusive, were made to a scale of one-twenty-fifth full size, and those of the ships from 260 feet to 160 feet long were made to a scale of one-twentieth full size. There is, of course, no special virtue in the absolute size of the ships the models are supposed to represent, nor in the absolute size of the models; the sizes adopted for the several models were those most convenient for construction and use, and the absolute size of the ships they are now taken to represent have been selected as being at the same time convenient multiples of the sizes of the models and as being rational sizes for actual ships; and the results which I shall presently give for this series of ships have been calculated from those of the models in the usual manner.

The models were tried stern first as well as head first.

It is worthy of mention that the models of the ships from 480 feet to 280 feet long, inclusive (and which were one-twenty-fifth full size), were all made from that of the 500-feet ship, by actually shortening it amidships (cutting out the necessary length of middle-body and rejoining the ends); and the models of the ships from 240 feet to 160 feet long (which were one-twentieth full size) were made from that of the 280-feet ship in the same manner. This was done partly for convenience and partly in order to insure identity in frictional quality of skin between the different members of the series.

The resistances of the series of ships I have described (calculated on the assumption that the surface of the ships is equivalent in quality to a surface of fresh varnish or paint) are all given in Figs. 3 and 4 in a manner which will be presently explained; but the resistances of a selection of these ships, head first, are also given in the more usual and more generally convenient form of "curves of resistance," in Fig. 2 more than half of the series being left out to avoid overcrowding the diagram. The ships whose curves of resistance are shown range from 160 feet to 480 feet in total length (and consequently from zero to 320 feet in length of straight side) by intervals of 40 feet. Their displacements range from 1,245 tons to 5,938 tons by intervals of about 142 tons.

Comparing together the curves of resistance of these ships, we find that at the lower speeds every added 40 feet of length (and 568.7 tons of displacement) increases the resistance by about the same amount; but at the higher speeds this harmony disappears. At 13 knots, for example, the 200-feet ship makes considerably more resistance than the 240-feet ship, which has 568 tons more displacement; and, though at 14½ knots the longer ship again makes the greater resistance, yet even at 14 knots the 280-feet ship makes less resistance both than the 200-feet ship of 1,137 tons less displacement and than the 240-ton ship of 568 tons less displacement; and at 14½ knots the 200-feet ship makes almost as much resistance as the 360-feet ship of 2,275 tons more displacement. Similar anomalies appear in the comparison between other ships. The tendency to alternate excesses and defects of resistance in the shorter ship as compared with the longer appears throughout the diagram.

Now, regarding the resistance of a ship as made up of three items, viz, skin friction, eddy-making resistance, and wave-making resistance, and remembering that the former is approximately proportional to the area of skin, so that addition of successive equal increments of parallel side can only affect it to the extent of producing corresponding equal increments for every additional length; the anomalies we have noticed can only be the result of some unexpected effect which the distance between the two ends produces upon the other two items, which make up what may be conveniently termed the "residuary resistance." To analyze properly the nature of this effect we must begin by eliminating the skin friction, the amount of which we believe we know by calculation. It also becomes our object to investigate not so much the effect of speed upon the "residuary resistance" of a given form as the effect produced on it by change of form at given speed, that change being the insertion of various lengths of straight middle body.

Accordingly, I have represented the results of all the series of ships, both head first and stern first, on Figs. 3 and 4, respectively, in a special manner adapted to this purpose, and it will be seen that here the apparent anomalies explain themselves completely. In the curves shown in these figures the ordinates above the zero line AA represent "residuary resistance," that is to say, total resistance minus the known resistance due to skin friction. The abscissæ represent not speed, as in Fig. 2, but lengths of parallel side. Thus the ordinates to the spots on the vertical line BB are the "residuary resistances" of the 160-feet ship having no parallel side, at the several speeds, 6.75 knots, 9.31 knots, 11.23 knots, 12.51 knots, 13.15 knots, 13.79 knots, and 14.43 knots. The series of spots next to the left indicate the "residuary resistances" at the same speeds of the 170-feet ship having 10 feet parallel side, and so on, the distances to the left of the zero line BB being length of parallel side on the scale of 20 feet to an inch.

Through the series of spots representing the "residuary resistances" of the series of models at each speed curved lines are drawn, each of which represents the gradual change in "residuary resistance" corresponding to gradual elongation at a particular speed, the ordinate to any one of the curves at any intermediate point between the spots being the probable "residuary resistance" of the ship having length of parallel side represented by the corresponding abscissa, at the speed belonging to the curve.

In the same manner the curves below the horizontal zero line AA represent the change in the surface-friction element due to elongation, the ordinates to these curves (measured downwards from the zero line AA) being at the stated speeds the skin-friction resistance of the ships having length of parallel side corresponding to the abscissa, so that measuring the total ordinate, from any of the spots representing "residuary resistance" of a certain ship at a certain speed down to the surface-friction curve for the same speed will give the total resistance of that ship, and so supply, if necessary, the information omitted from Fig. 2, as mentioned above.

Setting the skin friction aside for the present and considering the curves of "residuary resistance" only, we see that up to a speed of about 11 knots they are straight and level, showing that the residuary resistance is practically unchanged by insertion of parallel side, but that at higher speeds they present a series of regular undulations, showing that the gradual insertion of parallel side produces an alternate increase and diminution in the "residuary resistance."

These undulations present the following characteristics:

1. The spacing, so to speak, or length of the undulation appears uniform throughout each curve.

2. The spacing is more open in the curves of higher speed, the lengths being apparently about proportional to the square of the speed.

3. The amplitudes or heights of the undulations are greater in the curves of higher speed.

4. The amplitude in each curve diminishes as the length of parallel side increases.

Taking these graduated undulations in the diagrams of residuary resistance in terms of length of parallel sides, as an experimental fact, their existence at once harmonizes the apparent anomalies in the comparison of the curves of resistance exhibited by Fig. 2; for instance, taking the case of the 200-feet and 240-feet ships at the lower speeds, the "residuary resistance" being the same in both, the 240-feet ship has simply an excess of resistance equal to its increase of skin friction. At the 13.15-knot speed, however, the position of the 200-feet ship in the diagram falls near a summit, and that of the 240-feet ship near the succeeding hollow in the "residuary resistance" curve, and the consequent diminution in "residuary resistance" being greater than the increase of skin friction, the 240-feet ship comes to have the least total resistance of the two. At the still higher speed of 14.43 knots the summit of the curve comes about half-way between the positions of the two ships, so that the 240-feet ship, having no diminution in residuary resistance to counterbalance the excess of skin, has again the greater total resistance of the two.

Let us now examine the cause of these undulations. On Fig. 1, which shows the lines of the 500-feet ship, is a diagram representing on the same longitudinal scale, but for greater distinctness on a doubled vertical scale, the profile of the wave system, which, as seen against the side of the ship, would accompany it at the speed of 14.43 knots. This profile was obtained by actual observations made when the model was running at the corresponding speed.

This wave system consists of a series of crests at successive distances of about 125 feet, 235 feet, and 350 feet from the bow; with troughs between them at distances of about *180 feet, 295 feet, and 410 feet from the bow. Turning now to the diagram of "residuary resistance" at the same speed, we find that the successive hollows, or points of *minimum* resistance, correspond to total lengths of ship of about 168 feet, 277 feet, and 387 feet, and that the successive summits, or points of *maximum* resistance, correspond to total lengths of ship of 222 feet, 332 feet, and 446 feet. Now, if we deduct the figures just quoted as the several distances of the crests of the waves astern of the bow from the lengths of ship given by the successive points of *minimum* resistance, we find that these ships have a wave crest in each case about 40 feet ahead of the stern-post; and if we deduct the distances of the troughs of the waves astern of bow from the lengths of the ships having *maximum* resistance, we find that these ships have a wave trough about 40 feet ahead of the stern-post.

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* The wave length from crest to crest is just 115 feet, and this is almost precisely the length of an ocean wave having the same speed as that of the ship, viz, 14.43 knots or 24.3 feet per second. The speed of an ocean wave of 1″ period is just 5.09 feet per second, its length from crest to crest being 5.09 feet, and if γ be the speed of any other wave in feet per second, and λ its length, from crest to crest is $\lambda = \frac{\gamma^2}{5.1}$; 115,8.

This 40 feet or so is half the length of the after-body, so that the "residuary resistance" is smallest when the middle-body is of such length as to place the middle point of the after-body where a wave crest would be if the middle-body were continued, and largest when of such length as to place it where a trough would be. The inference is obvious that the undulations in the "residuary resistance" diagrams are due to the variations of quasi-hydrostatic pressure against the after-body, corresponding with the variations in its position with reference to the phases of the train of waves, there being a comparative excess of pressure (causing a forward force or diminution of resistance) when the after-body is opposite a crest, and the reverse when it is opposite a trough.

This circumstance at once explains all the characteristics of the undulations of these diagrams which were noticed above. Their spacing is uniform at a uniform speed because waves of given speed have always the same length; it is more open at the higher speeds because waves are longer the higher their speed; their amplitude is greater at the higher speeds because the waves made by the ship are higher, and their amplitude diminishes with increased length of middle-body because the wave system by diffusing itself transversely loses its height. It seems therefore impossible to doubt that the variations in position of the after-body, with reference to the wave system, is the sole cause of the variation in "residuary resistance" represented by the undulations of the diagrams in Fig. 3, and which produces such great apparent anomalies in the comparison of the series of curves of total resistance shown in Fig. 2.

This discovery is a most material addition to our conceptions of the manner of operation of wave-making resistance, and certainly serves to interpret many apparent anomalies in the curves of resistance of various forms. Hitherto our knowledge of the laws of wave-making resistance has amounted to little more than a crude appreciation of one broad principle, which underlies the most prominent manifestations of this kind of resistance. This principle is described in my lecture on "The Fundamental Principles of the Resistance of Ships," at the Royal Institution, in May, 1867, and I think I can not do better than quote the description here:

The waves [generated by a ship in passing through the water] originate in the local differences of pressure caused in the surrounding water by the vessel passing through it; let us suppose, then, that the features of a particular form are such that these differences of pressure tend to produce a variation in the water level shaped just like a natural wave, or like portions of a natural wave of a certain length.

Now, an ocean wave of a certain length has a certain appropriate speed, at which only it naturally travels, just as a pendulum of a certain length has a certain appropriate period of swing natural to it. And just as a small force recurring at intervals corresponding to the natural period of swing of a pendulum will sustain a very large oscillation, so, when a ship is traveling at the speed naturally appropriate to the waves which its features tend to form, the stream-line forces will sustain a very large wave. The result of this phenomenon is, that as a ship approaches this speed the waves become of exaggerated size, and run away with a proportionately exaggerated amount of power, causing corresponding resistance. This is the cause of that very disproportionate increase of resistance experienced with a small increase of speed when once a certain speed is reached, an instance of which is exhibited at a speed of about 13 knots in the curves of resistance shown in Fig. 2.

We thus see that the speed at which the rapid growth of resistance will commence is a speed somewhat less than that appropriate to the length of the wave which the ship tends to form. Now, the greater the length of a wave is the higher is the speed appropriate to it; therefore the greater the length of the waves which the ship tends to form the higher will be the speed at which the wave-making resistance begins to become formidable. We may therefore accept it as an approximate principle that the longer are the features of a ship which tend to make waves the longer will be the waves which tend to be made, the higher will be the speed she will be able to go before she begins to experience great wave-making resistance, and the less will be her wave-making resistance at any given speed.

This principle is the explanation of the extreme importance of having at least a certain length of form in a ship intended to attain a certain speed; for it is necessary, in order to avoid great wave-making resistance that the "wave features," as we may term them, should be long in comparison with the length of the wave which would naturally travel at the speed intended for the ship.

This view of the matter, then, recognizes the tendency of a ship, when the speed bears a certain relation to the length of her wave-making features, to make large waves and to incur corresponding wave-making resistance. But it does not take account of the possibility of the waves made by one feature of the form so placing themselves with reference to other features, as by the differences of pressure essential to their existence, either to cause an additional resistance, or on the other hand to cause a forward force which partly counterbalances the resistance originally due to their creation.

The way in which this may occur we have seen strikingly exhibited in the results of the experiments I have been describing. We see that in the very long parallel-sided form the sternmost of the train of waves left by the bow has become so small that its effect on the stern is almost insensible; and here we find, consequently, the united resistance due simply to the generation of a separate wave system by each end of the ship. As we gradually reduce the length of middle-body, the stern is brought within the reach of waves large enough to produce a sensible effect, and according as it is brought into conjunction with a crest or a hollow the total wave-making resistance becomes alternately less or greater than that due to the sum of the actions of the two ends of the ship when acting independently; the wave-making resistance becoming least of all (except at the very highest speed) when the middle-body is reduced to nothing.

This alternately favoring and resisting action of the train of waves also serves to explain one somewhat perplexing phenomenon which has manifested itself in the curves of resistance of many models of actual ships which I have tried, namely, the appearance in them of humps or contrary flexures. We have seen that the resistance depends on the relative placing of the after-body and the wave system; now the length spacing of the wave system (and consequently the positions of the troughs and crests) depends on the speed, and therefore the position of after-body, which is specially favorable at some given speed, may be specially unfavorable at a higher speed, and at a higher speed still may be favorable again. This may be seen by Fig. 3 to be the case, for example, with the 400-feet ship. The result of this alternation must be that comparing the curve of residuary resistance of, say, the particular ship we are considering, with the mean of the curves of residuary resistance of the whole series of ships, the curve of the particular ship will, at moderate speed, be below the mean curve—will, at a higher speed, rise much above it—and, at a still higher speed, will sink below it again; and if the mean curve be, as is probable, a fair curve, the ship's curve will necessarily present a hump in the middle. Such humps may be seen in several of the curves shown in Fig. 2, and these, on analysis, clearly arise from the cause I have been considering.

Although ordinary ships do not often exhibit a so markedly straight side as the series of forms we have been dealing with, nevertheless they frequently partake of this character quite sufficiently to introduce the operation of the causes we have been considering. Whatever the form of entrance, it must tend to make a considerable train of waves, as the speed appropriate to its length approaches, and whether the side be absolutely straight or gently rounded the position of the run with reference to this train of waves, will influence the resistance. Thus I think it certain that the two principal phenomena we have been examining, namely, the tendency to formation of a large train of waves at the speeds nearly appropriate to the length of the wave-making features, and the beneficial or prejudicial effect of the position of the after-body with reference to this train of waves, must form conspicuous elements in the curves of resistance of all forms at relatively high speeds.

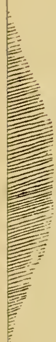
But the results of the present experiments point out, and to some extent evaluate, another important element of wave-making resistance besides those we have considered. I refer to the series of waves which diverge from the bow, the maintenance of which must of course involve resistance. None of these waves excepting the first impinge on the side of the vessel at all, and consequently the resistance due to them, unlike that due to the series of transverse waves, will be precisely the same whatever the position of the after-body. Now, looking at the diagrams on Fig. 3, we find that at the lower speeds the "residuary resistance" is unaffected by the length of parallel side. It cannot therefore be due to the formation of transverse waves, and the natural inference is that it is due to the formation of diverging waves.

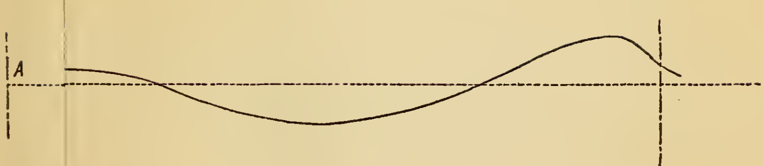
A corroboration of this inference is the fact that the diverging series of waves, though at high speeds small in comparison with the transverse series, becomes of great comparative importance as the speed diminishes, and is perfectly visible at speeds at which the transverse series is imperceptible. Again, this lower speed "residuary resistance," which is unaffected by length of parallel side, and which I consequently attribute to diverging waves, is markedly greater in the case of the stern-first series of forms, and this accords with the fact that the diverging waves of the stern-first series were visibly larger. At the same time, in attempting to estimate the exact amount of the resistance due to the diverging waves, it must not be forgotton that some portion at least of the "residuary resistance" may be due to eddy-making, although this must be comparatively small in a form of such fairly sharp lines, with stern and sternpost both finished off to a knife edge.

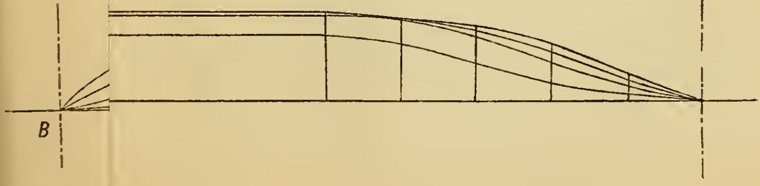
It seems probable that in other cases, as in this series, diverging waves are formed at speeds too low to produce transverse waves of importance, and that resistance due to the latter does not come into play until the speed of wave appropriate to the waves' features of the ship is nearly approached, when it begins to increase very rapidly. Consequently there may be a large class of cases where the speed is high enough to produce wave-making resistance due to diverging waves, but not that due to transverse waves; and where, therefore, the length of straight middle-body or its equivalent would not have much effect upon the "residuary resistance." Such ships, however, as the *Devastation*, *Fury*, and *Inflexible* certainly produce very marked transverse waves at full speed, and the phenomenon of the impact of the train of transverse waves upon the stern must doubtless constitute an important factor in the resistances of such ships.

Besides the conclusions of immediate practical importance which spring from the series of experiments I have described in this paper, I believe that a careful study of their results, in combination with our existing knowledge of the principles of wave motion, will ultimately throw most valuable light upon the details of operation on which wave-making resistance depends, and thus enable us to shape our experimental data concerning this element of resistance in a less empirical and more really instructive form.

Plate I.







To Illustrate Mr. Froude's Paper on the Wave-making Resistance of Ships.

FIG. 4.

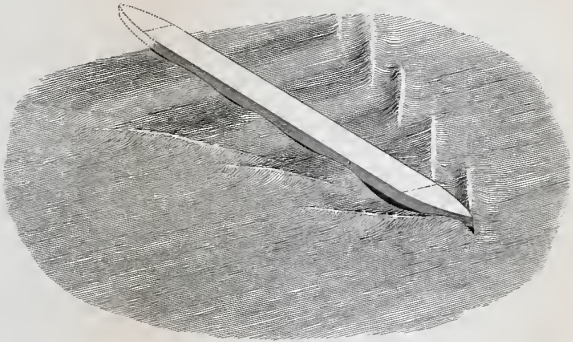
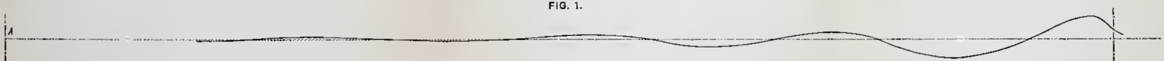


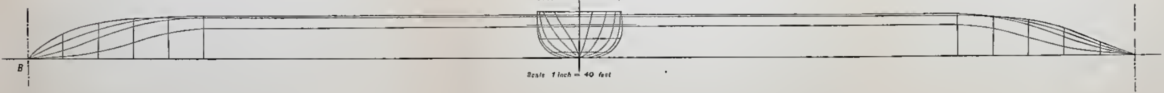
FIG. 1.



Displacements { Fore body — 598 tons; After body — 648 „; Middle body — 4832 „; Total — 6078 „ }

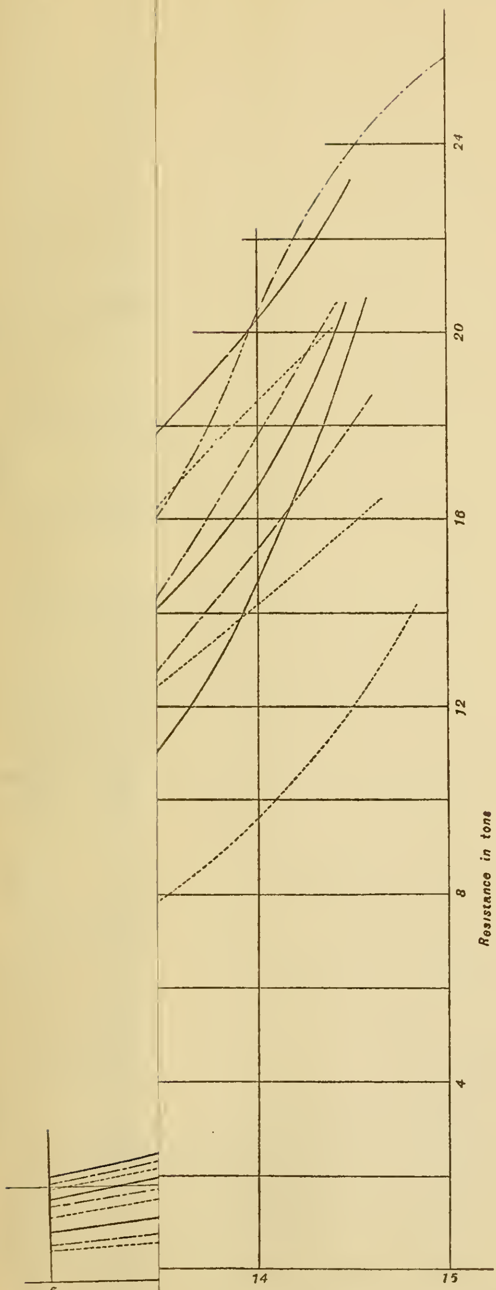
Principal Dimensions { Length extreme — 500.0 feet; Length of parallel middle body — 340.0 „; Breadth — 38.4 „; Draft — 14.4 „ }

Lines of 500 ft Ship



Scale 1 inch = 40 feet

Plate II.



To Illustrate Mr. Froude's Paper on the Wave-making Resistance of Ships.

FIG. 2

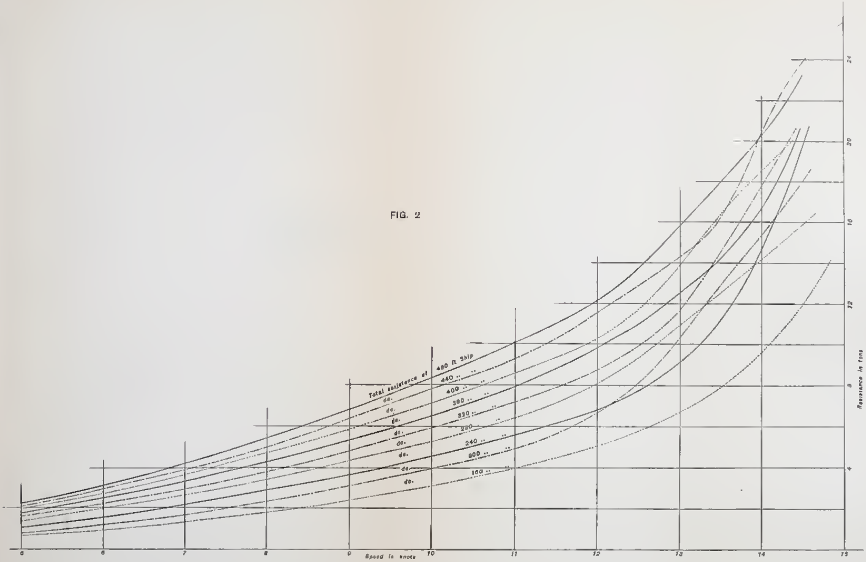
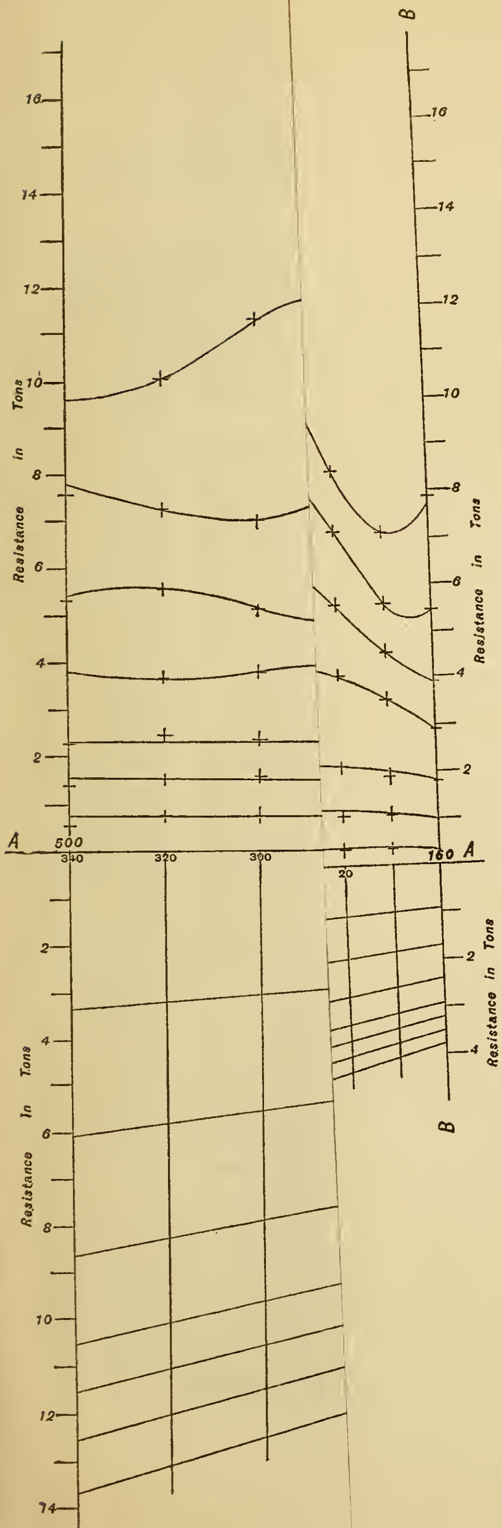
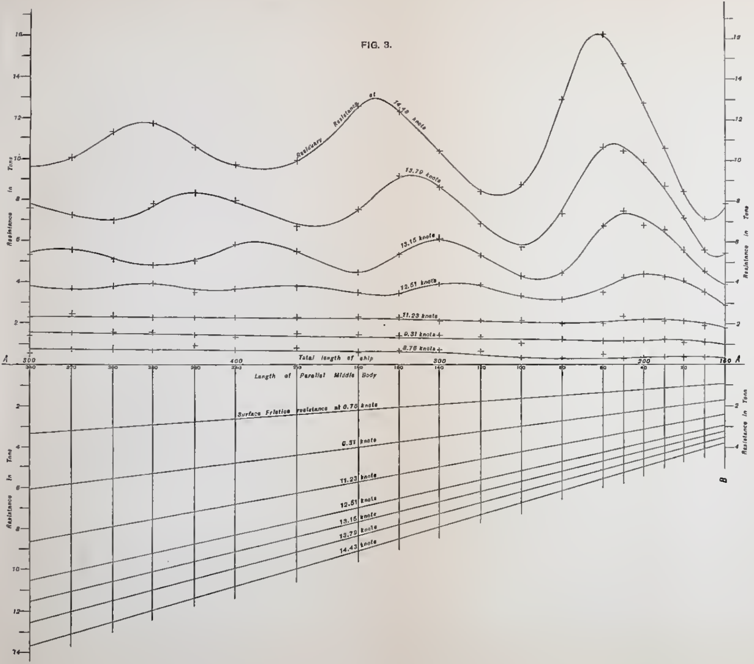


Plate III.



To Illustrate Mr. Froude's Paper on the Wave-making Resistance of Ships.



5.

LEADING PHENOMENA OF THE WAVE-MAKING RESISTANCE OF SHIPS.

By R. E. Froude, Esq.

[Read at the twenty-second session of the Institution of Naval Architects, 8th April, 1881.]

The purpose of this communication is to review the more salient points in the theory of wave-making resistance; and it will be convenient to take as a starting-point the paper on "The Effect of Parallel Middle Body," which was read before this institution by the late Mr. Froude, in the year 1877.

The experiments described in that paper were on a series of models, all having identical entrance and run, but different amounts of parallel middle body. The remarkable feature of the results was, that the introduction of the parallel middle body not only increased the skin friction in virtue of the added area of skin, but affected the wave-making resistance also in virtue of the changed position of the after-body in reference to the wave system left by the bow; so that if the parallel middle body were gradually elongated the wave-making resistance would alternately decrease and increase as the after-body was brought into favorable or unfavorable juxtaposition with the successive features of the wave system. This effect appears in Fig. 1 of the paper referred to, and is a synopsis of the results of the experiments.

"This discovery," as says the paper, was "a most material addition to our conceptions of the manner of operation of wave-making resistance." The theory, as thus completed, may be briefly sketched as follows: The passage of the features of the ship through the fluid involves local excesses and defects of pressure due to "stream-line" action, which tend to cause corresponding local rises and depressions of surface, thus forming undulations resembling waves or portions of waves. When the speed of the ship approximates to that appropriate to the lengths of these waves, large waves are formed, and proportionately great wave-making resistance is encountered. There tends, therefore, to be a rapid increase of resistance as a certain speed is approached—a phenomenon which is of course the more definitely marked the more nearly uniform are the wave-lengths of the several portions of waves which the features of the ship tend to form. But the part played by the waves is not necessarily complete with their original formation, for they are attended by "echoes" or following waves, which may increase or diminish resistance, according to their position in reference to the succeeding features of the ship's form.

Thus developed, the theory was seen to explain the peculiar irregularities observed in curves of resistance, and to enjoin, in the first place, that the features of the ship's form should, as far as possible, be long in comparison to the natural waves which would have the same speed as the ship; and in the second, that the echoes of such waves as are nevertheless formed by the earlier features should, at the intended speed, place themselves favorably in reference to the succeeding features.

It was recognized by the paper in question that the theory as thus presented applies only to the resistance involved in the formation of the kind of waves which have crests transverse to the line of motion; and that an important part is played by the series of "diverging" waves, of which I will speak in more detail presently. These, as opposed to the "transverse waves" first referred to, have the special characteristics (1) that their echoes or following waves do not touch the ship's side, so that their part in the resistance is completed with their original formation; (2) that their increase of size with increase of speed is more gradual, their apparent size and the resistance caused by them being well marked at moderate speeds, at which the "transverse" wave element appears altogether absent.

The principal purpose of the present communication is to suggest steps towards filling up some important details of the theory as above sketched, and to give some instances of how the main points of the theory manifest themselves in actual cases.

Wave-making resistance, regarded in its actual effect as experienced by the ship, is, of course, simply the net fore-and-aft resultant of the fluid pressures acting normally on all parts of the surface of the vessel. If a body is at rest in undisturbed fluid, the pressures are throughout the true hydrostatic pressures, and the net fore-and-aft effect is zero. If the body is traveling through the fluid, but deep below the surface, the pressures are largely changed from the hydrostatic pressures, in virtue of "stream-line" action, but still the net fore-and-aft effect is zero (except in so far as the equality between the favoring and resisting pressures is vitiated by eddy-making). If the body is traveling at or close to the surface, the pressure is still further changed from the mere hydrostatic pressures in virtue of the formation of waves, and such additional difference as is thereby introduced between the sum of the fore-and-aft pressures is the wave-making resistance. The approach to the surface of the fluid, by admitting of wave formation, has changed the pressures, because the wave system is really a changed set of stream lines, and involves a correspondingly changed set of pressures, the disposition of streams and pressures being throughout such that there is a perfect correspondence between the force acting on every particle and the motion thereby impressed upon it. It does not by any means follow that the change in the pressures is throughout in the direction of increase of resistance, *i. e.*, of increase in the sternward pressures and decrease in the forward pressures, for probably in most cases the change of pressure is of the nature of a large forward force on some parts of the surface and a larger sternward force on others, the wave-making resistance being the difference between these two. But it is, of course, an essential condition that this net sternward effect of the change of pressures should be such that the increase of energy consumed in propelling the body against the increase of resistance should equal the energy demanded by the maintenance of the wave system.

It will readily be understood that it would be fruitless to attempt to calculate, step by step, on first principles, even the approximate conformation of the combined wave and stream-line system that would attend the passage of any given body through fluid. But the converse process is comparatively easy, for we have sufficient knowledge of the character of the natural fluid disturbances proper to any given waves to form a tolerable idea of the nature of the enforced disturbances which would produce them. And if in this manner we learn what character of disturbance is implied by the waves which we see are actually made by ships, we obtain a clue to the solution of the question of how the passage of the several features of the ship's form through the water operates to produce the waves. A careful study of the actual wave systems produced is therefore an essential preliminary to the study of wave-making resistance.

In its main characteristics the wave system seems the same under all conditions. It seems invariably to consist partly of transverse and partly of diverging waves, and the angle of divergence of the latter does not vary greatly. The character of these two classes of waves is indicated in quasi-perspective in Fig. 4 of one of the illustrations of the 1877 Paper on Parallel Middle Body.

The transverse wave series consists of a row of parallel wave crests, square, or nearly so, to the line of motion, keeping pace with the ship, their length from crest to crest in the line of motion being about that proper to a deep water wave traveling at the same speed as the ship. In a very long, parallel-sided ship the crests of the transverse waves formed by the bow show for some distance against the side, successively diminishing in height as they spread sidewise, and seemingly also recede slowly from the side in virtue of any slight angle of divergence. If the parallel side is so long that these crests have in this manner disappeared by the time the after-body is reached, the stern is seen to leave a series of transverse waves of its own, of just the same character as that left by the bow; if, however, the straight side is not so long, these two series appear to coalesce into one.

The diverging waves present an instructive characteristic which escapes the observation of many persons whose attention is not specially called to it. If called upon to draw from memory a plan of the waves diverging from the bow of a vessel in motion, one would be inclined to show a long, continuous diagonal wave-crest, reaching far away from the ship's side, attended by perhaps one or two smaller crests nearly parallel to it.

To correct this erroneous conception we must imagine the smaller crest abolished and the single main crest cut up into short ridges, each of these ridges being stepped back from the line of crest of its preceding neighbor, so that they stand in a row, *en échelon.* They taper each way from the middle towards the ends, and the adjoining taper ends overlap one another somewhat. At any one speed these short crests retain unaltered their positions relatively to the ship and to one another.

The crests broaden and flatten towards their outer ends (*i. e.*, the ends furthest from the ship), so that the crest line here becomes quite indefinite, and from the shortness and general irregularity of shape of these crests it is difficult to measure with precision either their angle with the line of motion or their wave length normally to the crest line. The appearances, however, are perfectly consistent with what one would expect to find, namely, that the wave length is that appropriate to a wave traveling at a speed equal to the component of the ship's speed taken normally to the crest line.

The general line of the series, *i. e.*, a line drawn in plan through the highest points of the successive individual crests, diverges from the line of motion at an angle large enough to place clear of the ship's side all the diverging waves formed by the bow, except the first, and sometimes the very innermost end of the second.

The principal diverging series is formed at the bow, but a series precisely similar in character, though generally less marked, is formed by the stern also, and the two series may be traced, distinct from one another, for some distance away from the ship.

The angle of divergence of the diverging waves, and the relative importance of the transverse and diverging series, varies, of course, in different ships, and in the same ship at different speeds. The general characteristics of the system, however, as above described, seem common to all forms of vessels under all circumstances. It is instructive to trace step by step the train of modifications whereby the wave system accompanying a large ship at ordinary full speed, or its equivalent, that accompanying a torpedo-launch at low speed, changes into the system accompanying the same launch at her full speed. With this object a careful survey was made of the plan of the wave system accompanying a model of a torpedo-boat recently tried at Torquay at a large range of various speeds. At certain speeds, also, the longitudinal section of the level of the water surface in the wake was observed by measuring downwards from a carriage following the model on the level railway of the experiment tank. The results are shown in Figs. 3 and 4 (Plate I), the former showing the wave system made by an 83-feet launch at five speed, ranging from nine to twenty-one knots, the latter showing in comparison on the same scale the systems made at a speed of eighteen knots by the same 85-feet launch and by a ship of the same lines 333 feet long.

It will be seen that at the 9-knot speed for the 83-feet launch (or the 18-knot speed for the 333-feet ship) the wave system is precisely of the character observable in large ships at full speed, showing the familiar train of diverging waves at the bow and at the stern. As the speed is increased (or size decreased) both trains of diverging waves retain their character, but expand in scale relatively to the size of the ship, as they necessarily must (since the angle remains much the same), in order that their length may suit the speed; so that at 12 knots for the 83-feet boat the second diverging wave (*i. e.*, the first echo of the wave at the bow) is nearly opposite the stern; at 15 knots more than half a length clear of the stern; and at 21 knots nearly two lenghts clear. The point of departure of the first stern-diverging wave drops astern as the speed increases, and it becomes more acute at its forward end; at the higher speeds it is recognizable as the peculiar kind of flat-topped cliff of water, wedged-shaped in plan, which always appears immediately astern of a high-speed launch, and which is now seen to be the representative of the first member of the ordinary stern-diverging wave series.

It is interesting to notice that the bow-diverging waves made at 18 knots by the 83-feet launch and by the-333 feet ship, as shown on Fig. 4 (Plate I), are, to all intents and purposes, identical in length and relative position.

The transverse waves left in the wake were very low and flat at the high speeds, and were invisible to the eye (in the model) above the 15-knots speed for the 83-feet boat, but they show plainly in the longitudinal section of the wake at the 18 and 20¼ knot speeds, and are found to have the correct length appropriate to the speed.

Having now examined the characteristics of wave systems actually generated by ships, we have next to consider to what extent the various component parts of it are responsible for resistance.

The energy embodied in a wave of any given dimensions is easily calculable, and it would not be difficult to calculate the total energy represented by any system of waves or any parts of it. The question is not, however, what expenditure of energy must have been required to create the wave system, but what rate of consumption of energy is involved in its maintenance when created? The measure of this will be found to be simply the rate at which the wave system is essentially traveling away from the ship. True, in a wide sea the energy must eventually be absorbed in the internal friction of the wave motion; but we need not consider this, for the waste of energy of this kind in the immediate vicinity of the vessel must be infinitesimal, and as to the frictional waste in the rest of the wave system, it can only be operating in so far as a supply of energy to those parts of the wave system is being kept up from the parts nearer the vessel. It is the rate and manner of this supply, then, that has to be considered.

In reference to the diverging waves the solution seems easy. The wave as soon as formed immediately leaves the ship's side, and it is at once obvious that in running a given distance a ship must create a nearly equal length of new diverging wave on each side. The transverse waves, on the other hand, accompany the ship, and do not therefore at first sight seem capable of drawing away any energy, still less such large amounts of energy as are represented by the resistance which the experiment on parallel middle body proved to be due to them.

The explanation of this difficulty is also, as will be hereafter seen, the explanation of the curious skew arrangement of the crests of the diverging wave series. The explanation is to be found in the fact that a system of deep-water waves does not travel as fast as the individual waves composing it. In fact, the energy represented by the wave motions is transmitted from particle to particle in the direction of travel of the wave by the mechanical conditions of wave motion; but this transmission is only effected at half the speed of the individual waves, so that although in reference to the particles of water the energy is being transmitted forwards, yet in reference to the waves it is in effect draining backwards from each wave into its successor. Consequently, if a limited group or series of waves is traveling across an otherwise still surface, the leading waves are continually dwindling and eventually disappearing, for want of the energy they are leaving behind. The waves in the middle of the series are also leaving energy behind, but receiving an equal amount from the leaders, and consequently retain their size unaltered. The hinder ones receive from in front more than they leave behind, and are thus growing in size, while behind them new ones crop up. This may be seen happening in the wave rings formed by dropping a stone into water. Now, the wave system, as a whole, can only travel as fast as the energy is transmitted. The speed of the system, therefore, is exactly half that of the waves, so that in a wave system 100 yards long the wave which is at one moment coming into existence as the hindermost crest, by the time it has run 100 yards will have become the central wave, and in another 100 yards of run will be disappearing at the leading end.

If, then, it were not for the supply of energy afforded by the passage of the ship's form through the water, the transverse waves close to her would be continually dwindling and disappearing, and by the time the ship had run a given distance the foremost end of the wave system would be left half that distance behind. The work, therefore, which she has to do in maintaining the system intact is equivalent to that of continually lengthening the system at the rate of half her own speed, and the consequent resistance (since resistance is energy divided by travel) is the energy of a single wave divided by two wave lengths.

The proof of this proposition concerning deep-water wave systems has been drawn out by Prof. Osborne Reynolds in a paper read before the British Association in 1877, and also by Lord Rayleigh in his work on "The Theory of Sound."

I have said that this proposition is the explanation of the peculiar skew arrangement of the crests of the diverging series.

For let Fig. 5 (Plate II) represent the arrangement of a system of diverging waves at a given instant of time. A B is parallel to the line of motion. C D is one of the crest lines, and intersects A B at the point of E. F and G are the terminations of the wave system, of which the crest in line C D forms a part at E. Ten seconds ago, say, when the ship was a distance E E_1 astern of her present position, the crest line C D was at C_1 D_1, intersecting A B at E_1, and the terminations of the system were at F_1 and G_1. During the ten seconds' interval from then to the present moment, the point E_1 in the crest C_1 D_1 moved to H, but since the wave system and the energy resident at any point in it can only move at half the speed of the wave, the energy that was resident in the particle at E_1 will have only traveled to I, and the terminations of boundaries of the system to J and K. The parallel lines through F J and G K must therefore show the present position of the boundaries of the system, and the crest C D and the others parallel to it (as L L, N N, O O, etc.) must terminate in those lines. This it will be at once seen gives the skew arrangement already described as the characteristic of the diverging wave series. The line joining the highest points of the successive crests will of course be parallel to the boundaries, and it will be seen that the angle of this line with the lines of the individual crests has its tangent equal to half that of the angle of the crest lines with the line of motion.

Regarding the diagram as showing the positions, in space, of the series of waves at a given instant of time, we see that during the ten seconds preceding that instant the energy that was at E_1 has moved to I. So also, during the same time, the energy that was at every other point has moved a like distance in the same direction. If we now regard the diagram as moving along with the ship, instead of being stationary in space, it will represent the positions of the wave crests, not at one instant only, but continuously; the crest now at C D will therefore have been, ten seconds ago, not at C_1 D_1 but at C D, as now, and the energy now at I will have been not at E_1 but at E. The energy, therefore, during the ten seconds in which the ship was traveling a distance equal to E_1 E, will have traveled along the system, and relatively to the ship, the distance E I, and the energy that was at all other points in the wave system will also have moved an equal distance in the same direction. If, then, we take some other time interval, choosing E E_1 of such length that E I equals the distance between the crests along the line E I, we may say that the ship has to make one new diverging wave for every length equal to E E_1 that she travels. This gives as the expression for the resistance due to a given set of diverging waves:

$$R = \frac{E}{2 l \sin a,}*$$

where

$R =$ resistance in terms of any force unit.
$E =$ energy of a single wave in terms of the same force unit, and any distance unit.
$l =$ length of wave due to linear speed of ship, in terms of the same distance unit.
$a =$ angle of diverging wave crest with line of motion.

This, it should be noticed, by making $a = 90°$ (when $\sin a = 1$), ought to give, and does give, the same value for wave-making resistance of the series as has already been given for the transverse waves.

We now see, then, that the work a ship has to do in maintaining a wave system, whether of transverse or diverging waves, is in fact equivalent to adding to the system one new wave for every two wave lengths she travels. In this light the large resistance which we find to be incidental to the development of even a moderate train of waves becomes comprehensible enough, and we are now in a position to see why the point of equality between the speed of the ship and the speed appropriate to the wave she tends to form is not signalized by such an emphatic exaggeration of wave height as one might at first sight expect. In the late Mr. Froude's lecture at the Royal Institution, quoted in the paper on Parallel Middle Body, the effect of the equality above referred to is compared to the synchronism with the natural period of a pendulum of an alternating force applied to it; this analogy will be of use here. If a pendulum is nearly unresisted, we know that a very small alternating force, which produces no perceptible effect unless its period coincides almost exactly with that of the pendulum, will, if co-periodic with it, produce a large swing, so that the point of co-periodicity is very definitely marked by the result. But if the resistance of the pendulum is increased, the force required to produce a given swing in the co-periodic condition is increased, and the difference between this swing and that produced by the same force when not co-periodic becomes less marked. In fact, as the resistance increases, the importance of co-periodicity diminishes. Now, to represent the wave-making of a ship, where, as we have seen, the energy of one new wave has to be supplied for every two wave lengths of travel, we must suppose the pendulum to be subject to a resistance which would absorb half of its energy in every complete vibration.

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* If E I = crest distance along E I (as taken in text),

$$\frac{H\,I}{l} = \left(\frac{\text{speed of diverging wave}}{\text{speed of ship}}\right)^2 = \sin^2 a;$$

$$\therefore H\,I = l \sin^2 a.$$

$$\text{Now } E\,E_1 = \frac{H\,E_1}{\sin a} = \frac{2 \times H\,I}{\sin a};$$

$$\therefore E\,E_1 = 2\,l \sin a.$$

The analogy of the pendulum assists our conceptions of wave-making resistance in other ways. When the oscillation of a resisted pendulum is maintained by an alternating force, the alternating impulses must be given, not at the moment when the pendulum is at the ends of the swings, but while it is traveling in the direction in which the force acts; and if the forces are not discontinuous impulses, but change gradually so that their variations may be approximately represented by a harmonic diagram, the "phase" of this diagram of forces must anticipate that of the motion of the pendulum, the moment of maximum force in each direction being before the end of the swing of the pendulum in that direction. So that if we suppose stream-line pressures to be maintaining a wave or waves, it is clear that the "phase" of the pressures must precede the wave "phase," so that the points where maximum and minimum pressure would have been if the wave were not allowed to form would be forward of the crests and troughs of the waves which actually result. Applying this idea to the wave-making action of the bow of a ship with very long parallel side (see Fig. 6, Plate II); supposing the line A A to represent the heights due to the stream-line pressures which would have place along the surface of the bow if the wave were not allowed to form (as, for instance, if the upper works were removed and the surface of the water covered with ice), then the wave which would result would be not like B B B, a mere exaggeration of A A A, but rather like C C C, all the features being moved sternward.

The same reasoning applies to the action at the stern end, as indicated in Fig. 7 (Plate II). Now this sternward shift at both ends has a very important bearing on the question of resistance. We know from the stream-line theory that the stream-line pressures indicated by A A A (due to the supposed motion beneath ice) can exert no net fore-and-aft force upon the ship as a whole, consequently the pressures which would be represented by a wave surface such as B B B, which is only an exaggeration of A A A, can not do so either. But the sternward shift of the wave "phase" has the net effect of increasing the general pressure on the bow and diminishing that on the stern, and thus causes the resistance, without which resistance, as a means of extracting the necessary supply of energy from the ship, the resulting wave system could not live. Thus the difference between the curves A A A and C C C is at once the measure of the forces operating to cause the wave, and of their essential counterpart, the resistance of the ship.

The above reasoning applies to the independent wave-making operation of the two ends of the ship; let us next examine the operation on the stern of the train of transverse waves left by the bow.

We know, from the experiments on parallel middle body, that the position of the afterbody in reference to this train of waves affects the total amount of wave-making resistance, and that, according to this position, the total wave-making resistance may be either more or less than (or of course equal to) the sum o the wave-making resistances of the two ends acting independently. By what operation does this come about?

It is at first sight a reasonable view that the favoring or resisting effect is due merely to the general raising or lowering of the level of the water surface surrounding the afterbody, incidental to the presence there of one of the crests or troughs of the bow train of waves; the action of the afterbody to make waves on its own account going on meanwhile unimpeded. The bow train of waves in virtue of this favoring or resisting action are either restoring energy to or draining it from the ship, and their own energy must, of course, become correspondingly diminished or increased. It would seem, therefore, that the theoretically best possible result would be that in which the whole of the energy consumed in creating the bow train is re-absorbed at the stern,* thus annulling the resistance due to the formation of bow transverse waves, and leaving (in addition to that due to skin friction, etc., and diverging waves) only the formation of the stern transverse waves. The increase of resistance in the most unfavorable position of afterbody is due to a lowering of level, equal to the raising of level which produces the favoring result, so that the maximum possible increase ought to equal the maximum possible decrease. Therefore, according to this view, if we call the tranverse wave-making resistance of each end = 1, the total transverse wave-making resistance with very long parallel side would = 2, and the theoretical limits, so to speak, of goodness and badness of performance due to position of afterbody, would be—

in best position = 1
in worst position = 3

This view, which recognizes the restoration of the energy of the residue of the bow-wave series as the limit of its effect on the afterbody, may be called the "restitution" view. This view is plausible; but if followed out in detail is found to involve the following paradox: The afterbody if coming to still water (as at the end of a long parallel side) makes its own wave and the resistance appropriate to it. When the train of waves left by the bow is introduced in the most advantageous position, *i. e.*, that in which the whole bow-wave energy is restored to the ship at the stern, the above view assumes that the afterbody does its own work as before, unhindered by the presence of the bow wave. This might be reasonable, if nothing meanwhile happened to the bow-wave series at the afterbody, in which case the resulting wave system would be that ordinarily developed by the afterbody, added to that which came to it from the bow. But this can not be the case, for since the bow-wave energy has been restored, the waves representing it must have disappeared, and the resulting wave system must be that proper to the afterbody alone. We therefore have the paradox that

* This result could not, of course, be actually attained, because some of the energy must inevitably be lost by spreading sideways before the stern is reached.

the passage of the afterbody through the water leaves precisely the same resulting wave system, whether "fed," so to speak, at its forward end with still water or with water already in a state of wave motion.

To destroy or absorb the bow-wave system must require the action of forces derived somehow from the arrangement of stream-line pressures proper to the advance of the afterbody through the fluid, which arrangement of pressures is itself the source of the independent wave-making action of the afterbody. Since then both the absorption of the bow wave and the formation of the stern wave spring from the same source, it is impossible that the introduction of the former operation should not affect the latter. The bow wave in being absorbed must do something to the stern wave. It suggests itself as a reasonable hypothesis that this action is to arrest its formation; that, in fact, the function of the afterbody when advancing into water already in a certain state of wave motion is to swallow up that wave instead of making any of its own. If so, the placing of the afterbody in the most favorable position in reference to the bow wave series has a double benefit: (1) the bow waves restore their energy and are absorbed; and (2) in doing so they prevent the expenditure of energy in making stern waves. On this view the theoretically perfect result (*i. e.*, which would be obtainable with the best position of afterbody if the two ends of the ship were alike, and if the bow wave series did not spread away sideways before reaching the afterbody) would be that there would be no transverse waves left at all, and no resistance due to their formation. Therefore, if, as before, the united value of the transverse wave-making of the two ends acting independently = 2, the theoretical limits of goodness and badness of performance due to position of afterbody would be not 1 and 3, but 0 and 4.

The behavior of a pendulum affords an analogy which shows this supposition to be a rational one. Imagine a pendulum or plumb-bob fastened to a ring traveling along a rod at uniform speed. (See Fig. 8, Plate II.) Let the rod be bent transversely in two places to S curves, as at A A and B B, the two straight parts at each end being in the same straight line, and the middle straight part, A B, parallel to them.

When the ring travels on the part A A B B it will be first displaced sideways in one direction, will remain in this new position for a certain time, and be eventually replaced in its original position. The first displacement will get up a lateral swing in the pendulum (greater or less, according to the relation between the natural period of swing of the pendulum and the time occupied in the displacement), and this swing will continue unaltered as long as the ring remains on the middle straight part. This swing represents the transverse wave series left by the bow, which shows unaltered all along the parallel side, except so far as it diminishes by spreading sideways. If the pendulum be artificially stilled before the second curve arrives, the replacement will likewise generate a swing which will remain unaltered throughout the succeeding straight part and represent the train of independent transverse waves left by the stern in a vessel with very long parallel side.

But if, however, the pendulum remains swinging when the second curve arrives, the behavior of the pendulum on it, the magnitude of the resulting swing, and the total amount of energy expended in bringing the pendulum past the curves in the rod will all depend entirely upon the point in its vibration which it has reached at the moment of commencing the second curve. In this operation we have the analogue of the effect of the presence of the bow wave series upon the wave-making of the afterbody and the consequent dependence of the total resistance upon the position of the afterbody in reference to the train of waves.

What will be in all cases the behavior of the already swinging pendulum while traversing the second curve, and what will therefore be the resulting swing when it arrives on the final straight, seems at first sight a complicated question. In one case, however, it is very simple. It is perfectly clear that if the two curves, A A, B B, are exactly symmetrical with one another, and if the length of the middle straight is so chosen that the pendulum enters the second curve in the attitude and state of motion symmetrical to that in which it left the first, then the behavior of the pendulum throughout the journey over the second curve will be likewise symmetrical to its behavior on the first curve, and it must therefore leave the former, as it entered the latter, in a state of rest.

Here, then, in the action of the second curve to absorb the oscillation imparted by the first, its tendency to impart a swing on its own account being thereby defeated, we have a precise analogue of the suggested action of the afterbody, when most beneficially placed, to absorb the bow wave, and in so doing to forego making any wave of its own.

It is worth while pursuing the analogy of the pendulum further, and it may be observed that, by substituting a moving rod and a continuous string of stationary pendulums for the stationary rod and single moving pendulum, we may introduce the conception of resistance to longitudinal motion of the curved part of the rod, as the analogue of the resistance of the ship; and this resistance will be measured by the square of the amplitude of the swing of the pendulums left behind, just as the resistance of the ship, due to the formation of the transverse waves, is measured by the square of their height.

The case taken just now, in which the length of straight was such that the swing set up by the first curve was exactly quelled by the second, is really only a particular case of a very simple general proposition which for present purposes may be stated thus: If A A A (in Fig. 9, Plate II) represent the actual path of the pendulum bob in reference to the rod, B B B B what would be the continuation of this path if the straight continued (this being, of course, precisely similar to A A A), and C C C the path which it would acquire in passing over the second curve if it entered it without swing; then the actual resulting path will be such that the ordinate to it, at a point any given longitudinal distance from the line D D, will equal the sum of the ordinates (at points the same distance from D D) to the two paths B B, C C, the ordinates being taken in each case from the middle lines of the vibrations, and accounted as positive in one direction and negative in the other. In other words, the resulting swing will be the path B B, set off not from a straight line but from C C C, or, equally, it will be C C C set off from B B.

In fact, the resulting swing is a compound of two others, viz, that which would have remained if the second curve had not existed, and that which the second curve would have itself created if the previous swing had not existed; and these two imaginary component swings being themselves simple harmonic vibrations of the same period, it follows from the laws of harmonics that the actual resulting swing is likewise a simple harmonic motion of the same period as the two components. It also follows that when the two components are simultaneous in the same direction, the resulting vibration will be at its largest and will be the sum of the two, the energy being the square of that sum; and that when the components are simultaneous in opposite directions, the resultant will be at its smallest and will be their difference, the energy being the square of that difference.

Applying these propositions to the wave-making of a ship, they amount to this, that the combined or resultant wave series left behind the ship will be such remainder of the bow wave series as would have been there but for the after-body (and which may be called the bow component), "superposed" upon what might be called the "natural" stern-wave series, *i. e.*, the series that would have been made by the after-body if there had been no remainder of the bow-wave series (and which may be called the stern component). And therefore, when the two sets of crests coincide the resultant wave height will be greatest, and will equal the sum of the heights of the components; and when the crests of one component coincide with the troughs of the other the resultant wave height will be smallest, and will equal their difference. In any case the square of the height of the resultant series will represent the energy consumed in and resistance due to its formation. In the case already supposed, where the wave-making energy of both fore and after bodies = 1, and where the whole of that originally due to the fore-body is supposed to remain to be dealt with in the after-body, it is clear that, the two component wave systems being equal, the resultant wave height will in the best case be zero and in the worst case will be double that of either of the components and involve four times the energy. Thus the energy expended in transverse wave making will range from 0 to 4, as above anticipated.

The total expenditure of energy in transverse wave-making may be described as (1) the amount represented in the lost portion of the bow wave system (*i. e.*, that which has already gone away out of reach of the action of afterbody), added to (2) the amount represented in the combined wave system made by the after-body acting on the remaining portion of the bow system. Of this second item, the degree of coincidence between the two imaginary component wave systems affords the measure; not of course that the two components are supposed to exist and act upon one another clear of the stern and so regulate the height of the resulting system, but that their would-be coincidence is the criterion of what happens to one of the two when acted upon by the portion of the ship which would cause the other.

It has been pointed out that the theory as thus developed attributes a twofold operation to the bow wave series (or what remains of it) in its beneficial action on the after-body, viz.: (1) its own absorption, (2) the complete or partial frustration of the formation of the natural stern wave. In the case just now considered these two operations are equal. But it should be noticed that where, as must always be practically the case, only a part of the bow wave series remains, the second of the two operations grows very much in relative importance. Suppose, for instance, the bow wave series has dwindled to half its original height when it has reached the afterbody, then we may say:

a. Original height of bow wave series = height of natural stern wave series, say	$= 1$	
b. Energy of either (as before)	$= 1$	
c. Height of residue of bow wave series	$= \frac{1}{2}$	
d. Height of actual resultant stern wave in best case (*i. e.*, $a—c$)	$= 1 - \frac{1}{2}$	$= \frac{1}{2}$
e. Energy of ditto (*i. e.*, d^2)	$= (\frac{1}{2})^2$	$= \frac{1}{4}$
f. Energy of residue of bow wave restored by its absorption (*i. e.*, c^2)	$= (\frac{1}{2})^2$	$= \frac{1}{4}$
g. Energy saved by frustration of natural stern wave (*i. e.*, $b—e$)	$= 1 - \frac{1}{4}$	$= \frac{3}{4}$

Or, again, if the bow wave series had dwindled to one-third the height, we should have—

a and *b.* As before	$= 1$	
c. Height of residue of bow wave series	$= \frac{1}{3}$	
d. Height of actual resultant stern wave in best case (*i. e.*, $a—c$)	$= 1 - \frac{1}{3}$	$= \frac{2}{3}$
e. Energy of ditto (*i. e.*, d^2)	$= (\frac{2}{3})^2$	$= \frac{4}{9}$
f. Energy of residue of bow wave series, restored by its absorption (*i. e.*, c^2)	$= (\frac{1}{3})^2$	$= \frac{1}{9}$
g. Energy saved by frustration of natural stern wave (*i. e.*, $b—e$)	$= 1 - \frac{4}{9}$	$= \frac{5}{9}$

So that, in these two cases, out of the total effect of the twofold operation of the bow wave series already referred to, that attributable to the mere restoration of the energy of the residue of the bow wave accounts for only one-quarter in the first case, and one-sixth in the second.

The following, however, is really a more correct way of regarding the effect. Let A B be the height of the original bow and natural stern series respectively, and A^2 B^2 their energies. Let k A be the height of the residue of the bow wave series, and k^2 A^2, consequently, its energy. Then the actual resultant stern wave height in the best and worst position of afterbody will be $(B \pm k\ A)$ and its energy $(B \pm k\ A)^2$. Thus the total energy expended will be:

$A^2 - k^2A^2$ (*i. e.*, irrecoverable portion of wave formed at bow).
$+(B^2 \pm 2\ k\ A\ B + k^2\ A^2$ (formed at stern).
$= A^2 + B^2 \pm 2\ k\ A\ B.$

So that the term $k^2\ A^2$, which represents the energy saved out of the bow wave series, appears instead as part of the resistance of the stern wave, the actual favoring or unfavoring effect due to its presence being represented by the term $2\ k\ A\ B$, which is added to or deducted from the sum of the entire energies of the natural wave-makings of the two ends, represented by $A^2 + B^2$. As k diminishes, the term $2\ k\ A\ B$ increases relatively to $k^2\ A^2$, which is the only amount taken account of by the prima facie "restitution" view.

Some results of the experiments on models with various lengths of parallel middle-body throw a little practical light on the matter, and, so far as they go, they seem to corroborate the theory as above developed, and to negative the pure "restitution" view. These experiments, in addition to the measurement of the resistances of the models, with various lengths of parallel, which appear (reduced to ship figures) in Fig. 1 [of the 1877 Paper on Parallel Middle Body] included observations of the longitudinal section of the level of the water surface in the wake of several of the shorter models of the series. Now, a comparison of the resistances of the shorter models certainly seems to indicate, and a comparison of the heights of the waves observed seems to postulate, a greater alternately favoring and resisting effect, or in other words, a greater proportionate difference between the maximum and minimum amounts of transverse wave making resistance, than the "restitution" view will account for, even on the supposition that, when there is little or no parallel side, the whole of the energy of the transverse waves made by the bow remains available for absorption in the afterbody.

Again, it has been above pointed out that the height of the waves made and the amount of the resistance caused will be at the maximum or minimum according as the crests of the bow wave series coincide with the crests or troughs of the natural stern wave series. It follows also from the theory that in either of these two cases the crest of the resultant wave coincides with the crest of the larger of the two components, while if the crests of one series fall on the slopes of the other the resultant crest position will be a compromise between the crest positions of the components, though nearer, of course, to the larger of the two. Now, the wave sections observed in the wake of the models at the speed corresponding to 13.15 knots for the ships are shown (in ship figures) in Fig. 11 (Plate III) in a form which almost explains itself. They are shown above one another, the vertical interval between their base lines being proportional to the differences in length of parallel side, the points in each, representing the position of stern, being vertically over one another, so that the points representing the bow fall necessarily into a diagonal line. Diagonal lines parallel to this represent the position, as measured from the bow, of the successive wave crests of the bow wave series as they show against the parallel side in the longer ships. (These positions are taken from careful observations made on the longer models.) These diagonal lines may be therefore said to represent the position, in reference to the stern, of the crests of the bow component of the resultant wave series. Unfortunately, there are no trustworthy independent data for fixing the crest position of the natural stern wave, *i. e.*, the stern component, but there can be little doubt that it must be at the point where the crest position of the bow component coincides with that of the resultant, namely, in the line L. L.

It will be seen that the diagram shows pretty nearly what the theory prescribes, the maximum resistance and largest waves being about where the crest positions of the components coincide, and the minimum resistance and smallest waves where the crest of one falls in the trough of the other; further, we see that the resultant crest is not quite constant in position; but that where the crest positions of the components do not coincide it trends away slightly from the position appropriate to that of the stern component, towards that of the bow component.

We have now considered the operations of transverse wave-making of the two ends of a ship, acting both independently and in combination. There is yet one more theoretical point to which I will refer, and this has reference to wave-making generally. Imagine a flexible sheet or curtain floating vertically in still water. If this water were invaded by a regular series of waves, we should presently find the sheet, as the successive waves pass it, swayed from side to side and distorted so as to occupy alternately the two sides of a figure such as A A, B B, in Fig. 10 (Plate II). This figure has a definite character and may be said to be the same for waves of all proportions and sizes, with the proviso that its vertical scale is proportional to the length, and its transverse scale to the height, of the waves to which it is appropriate.

If for the flexible curtain we substitute a strong screen, forcibly moved at every point precisely as the curtain was swayed by the water, and in the same periodic time, it is clear that the screen will generate, in still water, forced waves precisely similar to those natural waves which previously swayed the curtain. Also that if the transverse scale of the figure through which the screen moves be varied (the character of the figure, however, as also its vertical scale and periodic time, being preserved unaltered), the height of wave generated will be proportional to the transverse scale of the figure, or, in other words, to its area.

If we substitute a different figure of the same area for the screen motion, keeping the same periodic time, it will still make waves of the same length as before, but clearly not of the same height; because, although the aggregate displacement of water will be the same, the figure being of the same area, the displacements do not locally correspond to those proper to the wave when formed, so that a screen, alternating with the same period through any figure of equal area but different character (such as a square or circle or ellipse or any irregular figure), or any figure even of the same character but differently proportioned vertical and transverse scales will make smaller waves.

Let C C D D be a figure of the same character, but of transverse scale n times, and vertical scale $\frac{1}{n}$ those of the figure A A B B; it therefore has the same area. C C D D will then be the figure through which a flexible curtain would be caused to alternate by natural waves having length $\frac{1}{n}$ times and period $\frac{1}{\sqrt{n}}$ times those to which the figure A A B B is appropriate. If, then, we substituted for this curtain also a screen forcibly alternated through the figure C C D D with period $\frac{1}{\sqrt{}}$ times that with which we have supposed the A A B B screen to be moving, then for similar reasons such screen so moving will make larger waves than a screen alternated through any other figure of equal area in the same periodic time. We see, then, that a screen alternating through a deep figure at a slow period and making the long waves appropriate to its depth will make larger waves and demand a larger supply of energy to keep it moving than one alternating through a shallow figure of the same area at the same slow period and making equally long waves; while, on the other hand, a screen alternating through a shallow figure at a quick period and making short waves, will make larger waves and therefore consume more energy than one moving through a deep figure of equal area at the same quick period and making equally short waves.

It is a reasonable inference from this that the wave-making features of a ship will operate more effectively to make short waves if their displacement is disposed broadwise rather than deepwise, and more effectively to make long waves if it be disposed deepwise rather than broadwise. Now, the diverging waves being necessarily much shorter than the transverse waves we see that flaring out the end sections of a ship or increasing the ratio of breadth to depth will, *cœteris paribus*, tend to increase the resistance due to diverging waves and diminish that due to transverse waves, while the giving U-sections or increasing ratio of depth to breadth will have the opposite effect. These inferences are visibly corroborated by the appearance of the wave systems caused in the cases referred to. Again, it is worth noticing that the experiments at Torquay have shown that as a rule moderately U-shaped sections are good for the forebody and comparatively V-shaped sections for the afterbody. This would seem to show that in the wave-making tendency of afterbody the diverging wave element is less formidable than in that of the forebody, and this inference corresponds with the fact that the stern diverging wave series is visibly less marked than that of the bow.

I now proceed to give some practical instances of the operation of the causes we have been considering.

Fig. 12 (Plate IV) shows resistance curves of long merchant ships of the usual type, models of which have been tried at Torquay. For convenience of comparison the ships are here brought to a uniform length of 400 feet. The other dimensions, displacements, etc., are tabulated on Fig. 12, and their load-water lines and a water line 10 feet below the load-water line for a distance of 120 feet from each end are shown together for comparison in Fig. 13 (Plate V).

To enable the features of the resistance curves to appear more distinctly the resistances are shown in the form of curves of "residuary" resistance, that is to say, total resistance minus skin friction. They include higher speeds than the vessels could attain, and for one of the ships at a lighter draught a curve is shown (on a different scale) up to 46 knots, at which speed the ship may be considered as corresponding to a torpedo-boat of 100 feet long, traveling at 23 knots.

I have introduced these results into this paper chiefly in order to exhibit the remarkable "humps" and "hollows" in the resistance curves, extending in regular succession, as we advance up the scale of speed even to torpedo-boat speeds.

These features begin to be visible at about 12 knots speed, and are very strongly marked at 16 and 18 knots. They occur at approximately the same speeds in each of the three ships and at each draught in the same ship. The speeds at which they occur are, however, rather higher, and the features themselves are more pronounced the deeper the draught. The combination of these two characteristics results in a curious difference between the effect of change of draught on resistance at different speeds. It will be found that all the above characteristics accord well with the theory as it has been presented in the earlier part of this paper.

In the paper on Parallel Middle Body it was pointed out that the operation of the bow train of waves upon the afterbody must not only produce at constant speed alternate excesses and defects of wave-making resistance compared to its mean value, as length of middle body is gradually changed, but where there is considerable length of parallel middle body must also, if length of middle body is constant and speed is varied, cause similar alternate excesses and defects of resistance compared to its mean rate of growth, and thus cause "humps" and "hollows" in the resistance curve. It was further surmised that these phenomena would manifest themselves in ordinary ships with long middle bodies, in spite of the fact that the side is generally rounded gently rather than absolutely straight; and this we now see to be the case. It will be readily seen that the theory must necessarily involve a relation between the speeds at which the successive "humps" and "hollows" occur. This works out as follows:

From the reasoning used above in connection with Figs. 6 and 7 (Plate II), it may be concluded that the principal wave-initiating operation of the bow of a ship is bestowed on the crest and back slope of the incipient wave, the succeeding trough and other features in succession behind it following naturally as parts of a wave system already formed. The same may be said of the wave-initiating operations of the stern, substituting, however, the word "trough and forward slope," and "succeeding crest" for "crest and back slope," and "succeeding trough."

As speed is increased the "succeeding trough" and "succeeding crest" referred to will both move sternward somewhat; but, probably, by an approximately equal amount, so that if the position of the "succeeding trough" at the bow be A, and those of the next crest, trough, crest, etc., be B, C, D, etc.; and if the position of the "succeeding crest" at the stern be A_1, and those of the next trough, crest, trough, etc., B_1, C_1, D_1, etc., the distances AA_1, BB_1, CC_1, DD_1, etc., which are of course all equal to one another, remain approximately constant in any given ship, whatever the speed. We may call this distance the "wave-system distance." Now, remembering that, according to theory, the wave-making resistance of the after-body is at its maximum when the crests of the independent bow and stern wave systems coincide and at its minimum when the crest of one coincides with a trough of the other, we should expect to get successive "humps" in the resistance curve (due to coincidence of crest with crest) at the speeds at which the "wave-making distance" = $\frac{1}{2}$, $1\frac{1}{2}$, $2\frac{1}{2}$, $3\frac{1}{2}$, etc., complete wave lengths, and "hollows" (due to coincidence of crest with trough) at the speeds at which the "wave-making distance" = 1, 2, 3, 4, etc., complete wave lengths. And since the length of a wave varies as the square of the speed the successive "hump" speeds will be:

$$\frac{C}{\sqrt{.5}},\quad \frac{C}{\sqrt{1.5}},\quad \frac{C}{\sqrt{2.5}},\quad \frac{C}{\sqrt{3.5}},\ \text{etc.,}$$

and the successive "hollow" speeds,

$$C,\quad \frac{C}{\sqrt{2}},\quad \frac{C}{\sqrt{3}},\quad \frac{C}{\sqrt{4}},\ \text{etc.,}$$

the value of the constant C being dependent on what we have called the "wave-making distance."

This reasoning is remarkably well verified in the curves on Fig. 12 (Plate IV). The speeds 13.7 knots, 16.2 knots, 20.9 knots, and 36 knots may be fairly taken as the successive "hump" speeds, and the speeds 12.8 knots, 14.8 knots, 18.1 knots, and 25.6 knots as the successive "hollow" speeds; and it may be seen that these eight speeds are severally proportional to the values:

$$\frac{1}{\sqrt{3.5}},\quad \frac{1}{\sqrt{2,5}},\quad \frac{1}{\sqrt{1.5}},\quad \frac{1}{\sqrt{.5}},\quad \frac{1}{\sqrt{4}},\quad \frac{1}{\sqrt{3}},\quad \frac{1}{\sqrt{2}},\ 1.$$

The fact already noticed, that the deeper the draught the higher are the speeds at which the "humps" and "hollows" occur, explains itself on the hypothesis that deepening brings more into play the fuller upper lines of the bow and stern, and thus by moving the bow wave system forwards and the stern wave system sternwards increases what we have called the "wave-system distance," and thereby increases the speed necessary to bring about a given coincidence between the independent wave systems of the ends.

Again, the "humps" and "hollows" in the curve of ship C are at rather lower speeds than in the other two ships. This seems attributable to her greater sharpness and hollowness of extreme bow, having the effect of diminishing the "wave-system distance."

Lastly, the fact that these hump features are more pronounced the deeper the draught is a witness that the transverse wave-making element, which causes these features, is increased by deepening of draught, and thus instances the greater potency of depth of form for transverse wave-making, referred to above.

DISCUSSION.

Mr. WHITBREAD. My Lord, may I ask Mr. Froude whether we could do away with the waves at the head of a vessel altogether, and fill up the vacuum at the stern so as to do away with the after waves, or dead water, or whatever it is termed? Would it not materially aid the vessel in her passage through the water if that wave in front and behind were removed?

Mr. FROUDE. Certainly, if the ends of the ship could be moved so as not to make the waves, it would materially aid the passage of the ship through the water. The question is, how to do that?

Mr. DENNY. I rise only for the purpose of expressing on behalf of those mercantile men who are owners of ships the great satisfaction this paper has given us. When I had the pleasure of listening to the paper referred to in it, read by Mr. Froude's late father, on the wave-making resistance due to additional lengths of middle-body in ships, I thought at that time it was hardly possible the subject could have been carried further, or that it could have been more completely exhausted. But the paper which has been read to us to-day has shown us that the subject can be carried further and that it can be still further exhausted; and we have reason to congratulate ourselves that, although in the loss of the late Mr. Froude we suffered a great loss, and one we all deeply regret, we have not suffered a loss made irreparable to us; but that in the place of the father we have the son, with all the promise which every one who studies naval architecture and desires its progress is glad to see. There is a point in which Mr. Froude resembles his father, which must be extremely grateful to builders in the merchant service. The late Mr. Froude took a deep interest in mercantile ship-building. He took a very deep interest in the question of the resistance of ships built for the mercantile marine. He experimented upon these, and not only experimented, but at great pains he helped to lay bare many of the fallacies which we now see exposed to view. I mean those fallacies in regard to extremely narrow ships. I think it is surprising that in so short a time seed of so good a quality should have produced such good fruit as the late Mr. Froude's labors have done in this way. In fact, the truth is that at this moment, so far as we in the mercantile marine think about speeds, we think in the thoughts and speak in the words of the late Mr. Froude, and we have comparatively no power to think beyond that. In the results Mr. Froude has placed before us to-day he has spoken of a ship 400 feet in length with a speed of 46 knots per hour. This, I think, is a matter worthy of note. If we have seen small vessels of 100 feet long brought up from a speed of, we will say, 9 or 10 knots, which launches used to go at, to a speed of 23 knots, is there not a hope, gentlemen, that we may see vessels 400 feet long brought up to the equally appropriate speed of 46 knots? I do not think, gentlemen, you will see that rapidly done. I think there is a great work before us before it will be possible to do that, but I think that Mr. Froude having given us this indication we should not allow it to drop out of our minds easily, but feel that we have a hope before us worthy of attainment in this diagram showing 46 knots speed.

Mr. Purvis. My Lord, one of the many advantages connected with this paper is the value that is shown to attach to experiments with models. I think the notion must be new to most in this room that four several trains of waves accompany a vessel, namely, one transverse series started by the bow, one transverse series started by the stern, one diverging series started by the bow, and one diverging series started by the stern. This must be new from the mere fact that the existence of these can not be seen in the case of a ship, owing to the disturbed state of the water; from one cause or another some of the phenomena are sure to be masked and not all appear. But Mr. Froude has brought before us in his paper far more than that. He has not only found out the existence of these series of waves, but he has found out the effect that two of them have in their mutual action upon the resistance of the ship. I think we shall all agree with Mr. Denny in what he has said, and feel thankful for the philosophic way in which Mr. Froude is conducting the experiments under his charge, and we look forward to more results in the future, perhaps of a more practical nature than those which he has been able to put before us to-day.

Mr. Barnaby. My Lord, I have heard with very great pleasure the remarks made by Mr. Denny as being the point of view of the advanced modern ship-builder as to the value of the work which has been done at Torquay, and I would also say that, on the other hand, the Torquay establishment benefits by the information placed at its disposal by those gentlemen who are engaged in producing the long merchant ships which have been spoken of here to-day. The models which have been experimented on there are models of ships which were placed at the service of Mr. Froude by ship-owners who are building ships at this moment for high Atlantic speeds. The names are not given here, but they are really the models of ships which are being built which are described in this paper, and the information gathered with regard to those forms has been communicated to the owners and the builders of those ships. I am quite sure I may say for Mr. Froude, as I say for myself very heartily, that we shall only be too delighted, so long as the Admiralty establishment at Torquay shall be the only one in England which is capable of doing that work, to have placed at our disposal information of that kind for the purpose of making experiments such as those which Mr. Froude has so ably described this morning. We can only hope that the efforts of Mr. Denny, and those who are engaged with him, to establish similar places in other parts of England may be successful; we heartily hope they may be. One point which is very interesting to me as the representative of those who are concerned in the building of ships of war is the point which has been referred to by Mr. Denny, namely, the advantages which are, apparently, to be secured by having greater beam given to the ships. I never attempt to look into the future regarding ships of war, costing such large sums as those ships do, in order to discover, if I can, what the future has to show, what the guns are going to be, what the armor is going to be, what the size and the cost of those ships are going to be, without falling back upon this reflection, that in the end we shall have to look very largely indeed to the mercantile shipping of the world to protect and look after its own interests. One of the difficulties in the consideration of that question is the extreme narrowness of the merchant ship. We know perfectly well that we can not ask the mercantile ship-builders and ship-owners to put their machinery below the water-line. They will have engines extending a very long way above the water. We never can look upon machinery arranged in that way, in such ships as we see crossing the Atlantic, without shuddering at the reflection that if they were exposed to shot or shell-fire, that machinery would very speedily, in all probability, receive serious injury—that, in fact, it is necessary to give to it some protection. Protection can not be given to that machinery above the water unless there is considerable beam to the ship. The ships as they are now built are too narrow for the machinery to receive that amount of protection which it ought to receive if the ships are ever to perform the service which I can not help regarding as the service which they will have to perform in the future. There is another point to which attention has been called, and that is, the possibility of having these very high speeds. My friend who is sitting here on my left, and who was the first to show us the remarkable phenomenon of getting boats to rise up out of the water and travel along at amazing speed (Mr. Thornycroft), has, I know, devoted his attention to the question whether it may or may not be possible to make big ships do the same thing. I think he came to the conclusion that he would want tremendous horse-power in his engines, and that there did not seem to be any way at present in which to get that. One very encouraging thing in regard to that is to see how those curves as you get to those high speeds flatten out and run away. It may be gratifying to you to know that it is not therefore an impossibility, and that we may be talking this morning about a thing which may really happen before very many years are over our heads. Perhaps while your Lordship is president of the institution we shall have sea-going ships doing what Mr. Thornycroft has shown us can be done with boats. Let us hope that that may be so.

Captain CURTIS, R. N. Mr. Froude has suggested that the U form for the entrance or mid-section, and the V form for the run, is desirable for speed. In 1853 I was on the coast of Africa, and as we were sailing along, I always found that a porpoise went faster than the ship, at whatever speed the latter was going, and crossed her bows. It occurred to me that I might make use of the formation of the porpoise. I took a pair of compasses and took the lines. In 1854 I transferred that to paper, and the result was very nearly the same as the section of a salmon, and also of a mackerel. I would call your attention to the lower diagram, in which I will just refer to the black line about two-fifths from the bow. For instance, if a vessel was about 30 inches long, one-half the length of a porpoise, her extreme beam would be at 12 inches from the bow; it would be $7\frac{5}{10}$ inches beam at 12 inches from bow. I may state that the midship section at two-fifths is a semicircle, and every section of that vessel is a segment of the same circle. Mr. Froude will correct me if I am wrong, but I think that is the form that he requires in order that no other part of the ship except the bow should create a wave; and the run or afterbody should absorb the bow wave as far as possible. The bow is always creating a wave. I think that is all I have to say. It bears out the statement that the extreme beam should be at two-fifths of the length: Beaufoy's experience.

Mr. BILES. My lord, with respect to the remarks of Captain Curtis, I think if we were designing ships to go under the water instead of to go on the surface of the water, it would be very valuable to have the results of observations made upon fish laid before this institution, but as the experiments of Mr. Froude have been made upon models which run upon the surface of the water, such observations seem hardly relevant to the question. This question of wave-making resistance is part of one great question, and I do think it is one that has not received the attention that it should have. We have had several papers during the present meeting, and in the last two meetings, upon the question of stability. Now I think, my lord, that the question of stability is one that we may certainly say is well in hand. We know how to make ships safe, but with respect to this question of wave-resistance there is so much yet to be learned that we may say that we know comparatively nothing about it. It is, therefore, one that this institution should devote its energies to, because the interests that are involved in this question of wave-resistance are so great and so wide that really the results to be reaped from such an investigation would repay an almost infinite amount of labor and outlay. I think that this question has not been brought so prominently before this institution as it should have been. Mr. Froude stands, as Mr. Denny has pointed out, almost alone in his investigations upon this question; and it is a great thing, and a thing which this institution should congratulate itself upon, that the ship-builders—some of the leading ship-builders at any rate—are following on in the lines of Mr. Froude, and endeavoring to supplement the information which he is giving by results of actual experiments—actual trials, I should say—with big ships. The peculiarities shown there (pointing to diagram) in those curves of resistance do not present themselves upon the speed curves obtained at progressive mile trials, and I think the reason of that is, partly, that speeds which have been obtained in vessels have not been so high as shown there, and partly from the fact that a sufficient number of speeds has not been taken in order to determine them as closely as they are determined there. I suppose it would be impossible to determine those curves unless speeds were taken at half-knots apart. But I think it would be possible, without going into the time that would be required to carry out the speeds at every half-knot of the measured mile, to determine those curves a little more accurately than is done on the mile. And it would be done in this way: If a speed curve were plotted in the usual way in which it is in the results of ordinary progressive mile trials, and the revolution curve were then placed alongside it, it would be easy afterwards while the champagne was being drunk, and the pleasure party was going on as is the case during these trials, to take a series of observations merely by taking revolutions and indicator diagrams, and plotting those on the basis of revolution, and not speed, filling in the curve in much greater detail than it is filled in at present. I think that is a point which is worthy of the notice of those ship-builders who go to the trouble and expense of carrying on progressive mile trials. Mr. Froude has spoken with reference to the diverging waves, and has said that it is somewhat difficult to notice those waves at this point, but I had occasion lately to notice a series of waves in a paddle-boat. I have the permission of Mr. Thompson to say that we shall be trying a paddle-boat again in the course of a week or two at a high speed, and he will be very happy to carry out any experiment which Mr. Froude may indicate, with a view to observing the large diverging waves which are shown in the course of these paddle-boat experiments. The investigation which Mr. Froude has laid before us of the way in which the waves die out is an exceedingly elegant one, and I think it is one for which this institution can not thank him too highly.

Mr. W. H. White. Mr. Barnaby has spoken of the hopefulness of the future as regards the attainment in full-sized ships of very high speeds. I have taken the trouble while Mr. Biles has been speaking to work out the horse-power corresponding to the 46 knots speed for a vessel of 5.400

tons. I have the authority of Mr. Froude for saying that if we take the residuary resistance only, and leave out the surface friction, we shall require the modest indicated horse-power of about 130,000. Of course, I am quite at one with Mr. Barnaby about the hopefulness. I simply want to indicate the magnitude of the undertaking. I quite agree with Mr. Barnaby there is hope for certain purposes. But it is a question for the engineers chiefly. How can they give us the power, and how can we use the power? For it will be wanted, so far as I can judge. Mr. Thornycroft, or Mr. Yarrow, or anybody else who is skilled in engineering, will have to discover how about 130,000 horse-power can be usefully applied to a vessel of that draught—we need not be particular, because when we get up to that power, or a little more, we can begin to consider the question. But, at present, all that we can do is to look at it, as Mr. Barnaby says, hopefully, always remembering that the corresponding problem has been solved in a smaller vessel. Now that brings me to what I want to say. Mr. Barnaby had in his mind, I know, because it is a fact with which he is perfectly familiar, that when these very high speeds are reached the increase of power is just as the speed; that is to say, the curve of resistance turns into a straight line. In the torpedo-boat that condition is reached. When Mr. Bramwell read his paper here, I analyzed the curve of the *Miranda.* I did not quite believe it; I thought there must have been some mistake. There were acknowedged difficulties in that case in measuring the power. I doubted the curve simply on that account. Now we know, as a matter of fact, that if those speeds can be reached where, as Mr. Barnaby says, the vessel comes on to the top of the water, the expenditure of power for higher speeds increases at a moderate rate. But before that is done we shall want a small revolution in our engineering. I would join in the thanks which have been given to Mr. Froude for his great labor, not merely in the preparation of his paper, because that does not represent it at all, but in the experiments that went before. When we look at these beautiful drawings, and we know, as I do personally, that for every indication on those drawings a series of observations and check observations has to be made—I do not know how many journeys must be performed up and down the tank to make sure that they are right—we must be very grateful to any gentleman who will undertake the extreme labor and the careful observation necessary to produce diagrams like these. I can never look at these diagrams without thinking of the changed conditions since Professor Rankine first began to conduct his analytical investigations into this question of resistance and wave-making. Not long ago I had occasion to go through that part of "Ship-building theoretical and practical" where the accounts of his investigations are given, and it is singular to notice how fully he appreciated the existence of those diverging waves; but I do not think he dreamt of the existence of transverse waves, so far as I have been able to trace his reasoning. If Professor Rankine, with his wonderful power of analysis, had had such facts as these to work upon, we can not help seeing that his labors would have been productive of much greater influence on practice. As a matter of fact he could only make observations on ships, and Mr. Inglis conducted most of the observations that were made in connection with these diverging waves. You will find the facts stated in our transactions in connection with Professor Rankine's paper on virtual depth. Now in contrast to that analytical method—that draught upon the imagination which Professor Rankine and those who worked in the early days had so largely to make—we have here introduced well-established facts which Mr. Froude has shown how to analyze. We first get the facts, and then we attempt to account for them and to make use of them. The change of conditions, gentlemen, it seems to me, is enormous; and as speeds go on, and we get beyond what precedent can teach, and have to work under entirely novel conditions, I can not help thinking that this great system of model experiments which Mr. Froude has grafted upon Professor Rankine's stream-line theory, is the thing to help us more than anything else.

Mr. JAMES HAMILTON, jr. My Lord, I desire to make a single observation with reference to diagrams Nos. 3 and 4. Mr. Froude has pointed out that diagram No. 4 shows the same wave formation for a very small steam launch as for a large ship. I think that is what one would expect if the forms are similar and if they are driven at corresponding speeds. But if you look at figure 3, I think it is surprising that the wave formation of the small launch should be the same at 9 knots as it is at 21 knots. If my eye is correct it seems to undergo no change of formation as the speed is increased; the angle of the diverging wave being the same, making the same angle at 9 knots as at 21 knots. In three of the vessels which I referred to yesterday in my paper, the angle of the diverging wave was very carefully noted at the time of the trial, and it showed a very perceptible difference as the speed increased. In the fullest of those ships it ranged from about 25 degrees at the slow speeds to 49 degrees at the highest speeds. In short, it was about doubled. Now I should like to ask Mr. Froude if he considers this case to be a representative one or is a special case? If I might be allowed, I would suggest that Messrs. Thompson at their coming trial should carefully note the angle of the waves at the different speeds.

Mr. FROUDE. I understand you to say that the angle was more abrupt at the high speed.

Mr. J. HAMILTON, jr. Much more abrupt, 49 degrees.

Mr. FROUDE. Yes; I wanted merely to make sure of the point. In replying upon this paper, I wish first to express my thanks to Mr. White for suggesting that I should prepare the paper. If it had not been for his suggestion I should not have done so. Mr. Barnaby also pressed me to do so, and it was also Mr. White's suggestion that these observations on the waves should be made with a torpedo-launch model. I am not sure that otherwise I should have made them. Probably, if I had been pressed for time, or there had been any doubt about it, I should have omitted them. It was Mr. White's particular request that they should be made, but I think myself that they are one of the most interesting points in the paper. I wish to acknowledge that in that respect I am indebted to him. When it was suggested to me it at once awoke my own interest in the matter. I felt exceedingly curious to know, in the first instance, what that peculiar wave is which is immediately in the wake of the launch, close to the stern. It was a surprise to me when I found it was simply the ordinary stern-diverging wave series, adapting itself to that condition. With respect to what Mr. Hamilton said, I was myself surprised to find that the angle of the diverging waves was in this case so constant at different speeds. According to the observations they seem nearly constant; but, as I mentioned in the paper, it is exceedingly difficult to say what the angle of the diverging wave is. The form it takes is that of a very small crest, which extends for some distance without increase of size; then it becomes more marked, but as soon as it obtains its maximum height it turns into a rounded shape something like a mussel shell, as I have endeavored to represent on the diagram. Certainly the general line included a minute crest which precedes the rest and forms a curve which you can not say has any definite angle at all. Still the positions of the crests seem to indicate that the angle of the series in this case is constant. In the launch, of which the diagrams are given there, the angle seems to be much more acute—that is to say, more nearly parallel to the line of motion—than they usually are in my experience. Possibly, if we had tried blunter models in the same way we should have got a blunter angle throughout, and possibly an angle which would have changed more with the speed. This is only a particular case, and one can not say it would be so constant in all other cases. I quite indorse what Mr. Biles has said as to the non-appearance of the humps in the resistance curves on progressive trials. No doubt the explanation of the humps not appearing in the progressive trials is, that the observations are not taken at sufficiently close intervals of speed. At the same time the speeds shown on these diagrams are higher than ships commonly attain, and the humps are not plainly marked at moderate speeds. Certainly I think that to try more speeds, and to use the revolutions as a measure of speed, would be a very interesting addition to the ordinary form of progressive trial. I have felt exceedingly flattered by the remarks of Mr. Denny, and of Mr. Barnaby; and, I confess, I am surprised to find the paper is regarded with so much interest. It seemed to me that it is not only theoretical, but that a good deal of it is hypothetical, and is very incomplete. You may call it dough; one may hope it may become bread some day. A great part of it requires a good deal of treatment before it can be made of practical use. I was very much pleased that Mr. Denny was struck favorably by the appearance of the curves of speed up to 46 knots. I made out those curves for ships of moderate scale, in order that the speeds might not seem so frightening. Now, I almost wish I had made them out for a larger ship, since it is considered so encouraging to see these high speeds represented.

The PRESIDENT. Gentlemen, I am sure I can convey, with your approbation, our united thanks to Mr. Froude for his most able paper, and we have had a most interesting discussion to follow upon it. Mr. Denny, in those very touching, and I think beautiful, remarks that he addressed to us, probably paid as high a compliment as he possibly could do to the memory of our lamented member, Mr. Froude, and to the labors of his useful life, when he told you that ship-builders think in his thoughts. It was a beautiful phrase, and it adequately described the fact. And I think that his worthy son—"the worthy son of a worthy sire"—has given great encouragement to active minds such as that of Mr. Denny himself, and his able assistant, who has addressed us with so much ability to-day. He has given them good promise that those thoughts shall not repose. Mr. Froude is following in the steps of his father, and we all know that science never stops; and it is impossible to say, if his life be long prolonged, as we all hope, to what extent he may carry those thoughts. It is also, I think, due that we should recognize publicly the gratitude of the public to the Admiralty for these most valuable experiments that are being carried on upon the great subject of wave resistance. It is eminently one which is occupying the thoughts of our ship-builders more than any other at this moment, and it must be a satisfaction to all of you to have proof of such ability as Mr. Froude displays and exercises in watching and conducting these experiments. I convey, I am sure with your full appreciation, your thanks to Mr. Froude for his paper.

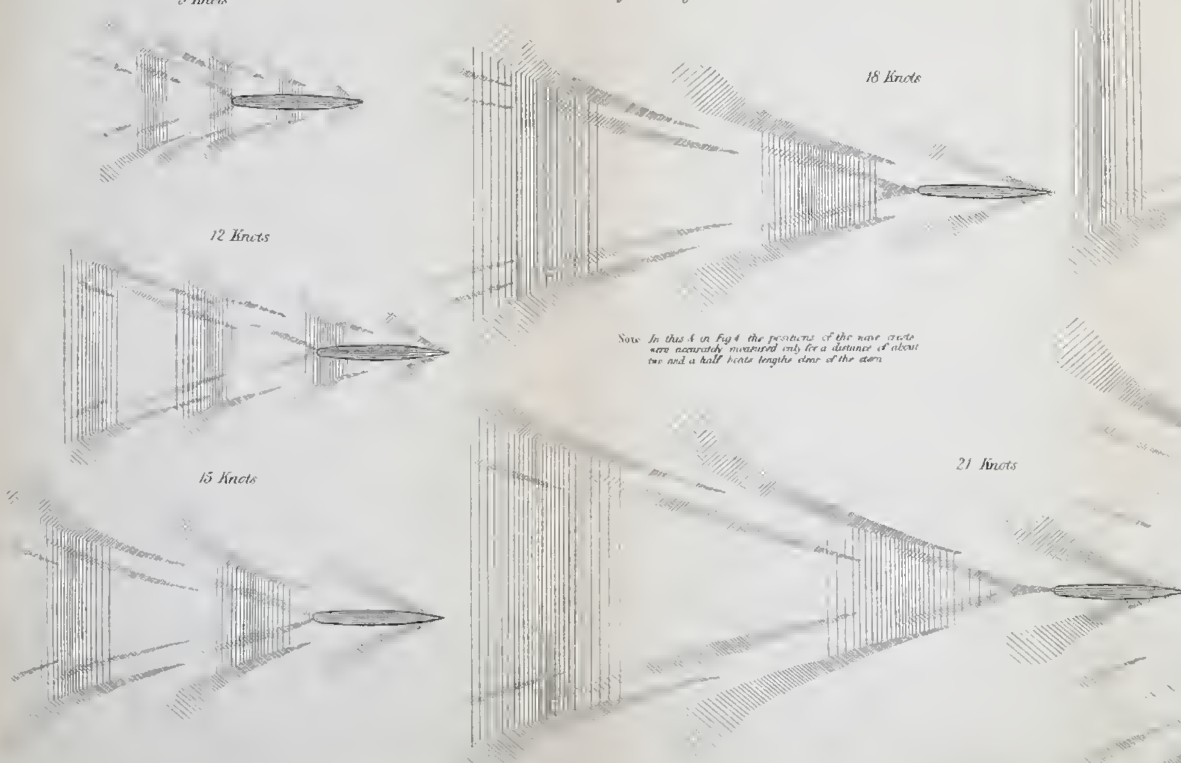
To Illustrate Mr. R. E. Froude's Paper on the Leading Phenomena of the Wave Making Resistance of Ships

FIG 3

PLAN OF WAVE SYSTEM MADE BY 83 FT LAUNCH AT VARIOUS SPEEDS

SCALE 80 FT TO AN INCH

N.B. Position of Wave Crests indicated by Shading



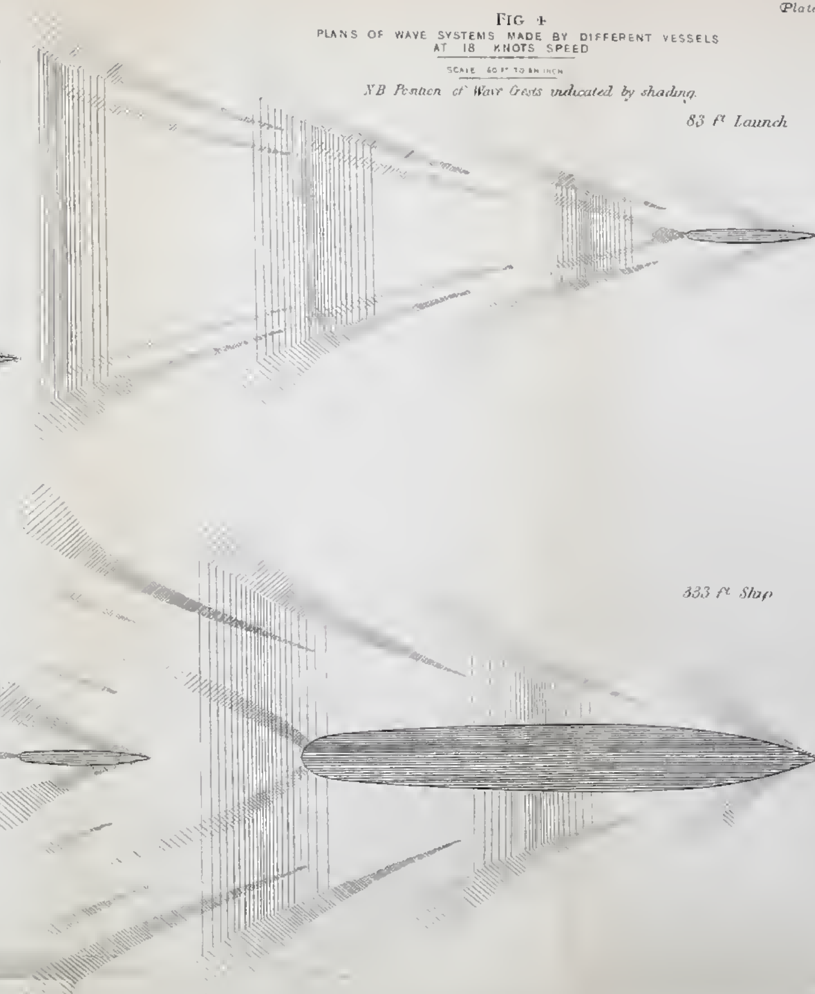
Note. In this & in Fig 4 the positions of the wave crests were accurately measured only for a distance of about two and a half boats lengths clear of the stern

FIG 4

PLANS OF WAVE SYSTEMS MADE BY DIFFERENT VESSELS AT 18 KNOTS SPEED

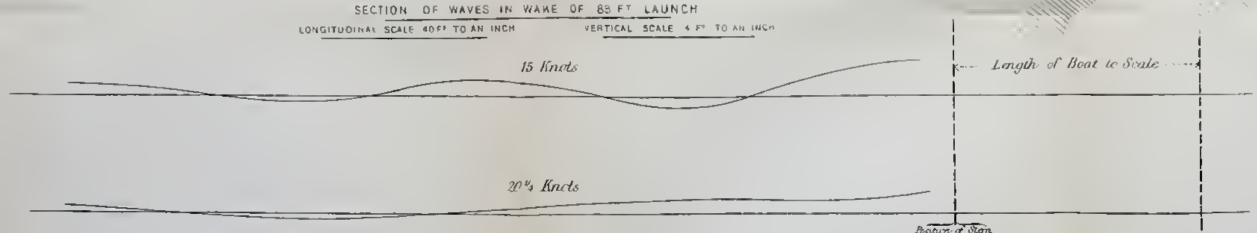
SCALE 80 FT TO AN INCH

N.B. Position of Wave Crests indicated by shading.



SECTION OF WAVES IN WAKE OF 83 FT LAUNCH

LONGITUDINAL SCALE 40 FT TO AN INCH   VERTICAL SCALE 4 FT TO AN INCH



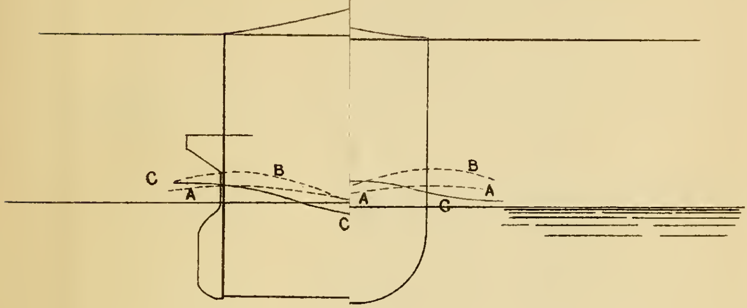


FIG 10.

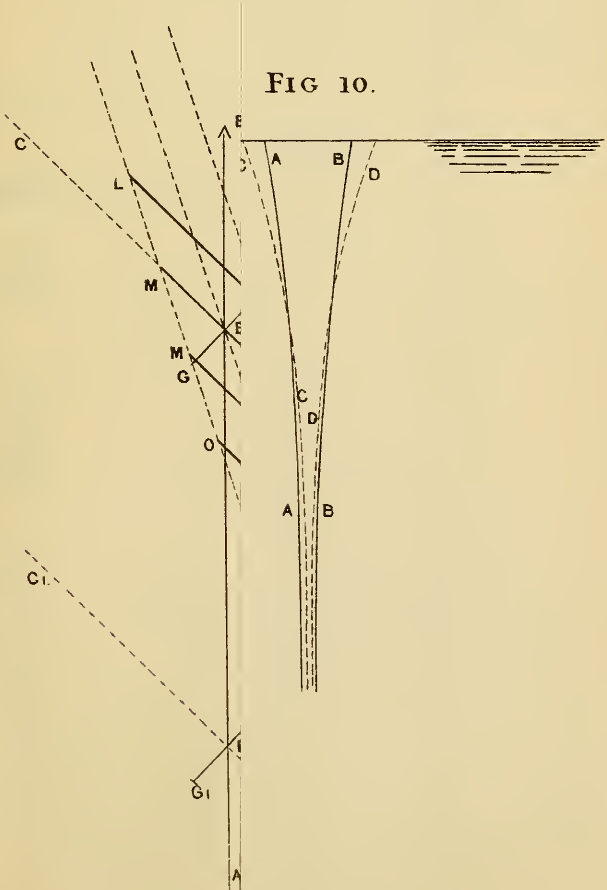


FIG 7

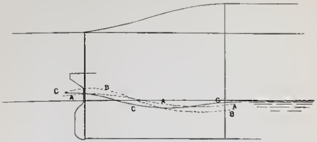


FIG 6

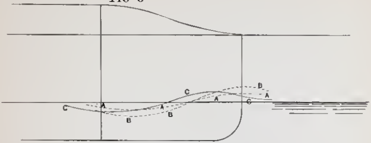


FIG 9

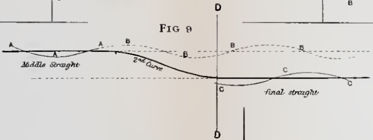


FIG. 5

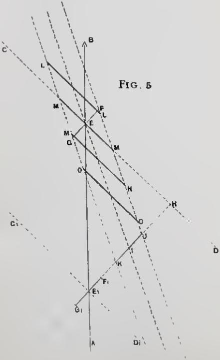


FIG 8

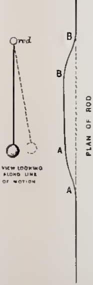
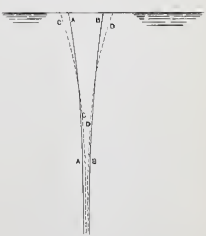


FIG 10



Vave N

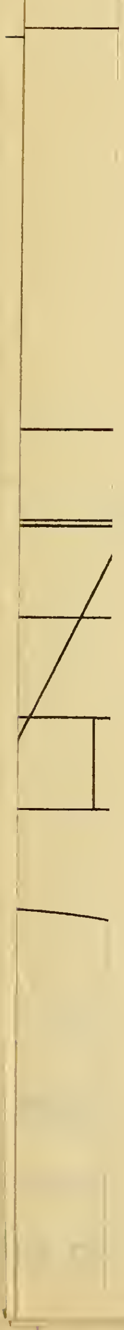
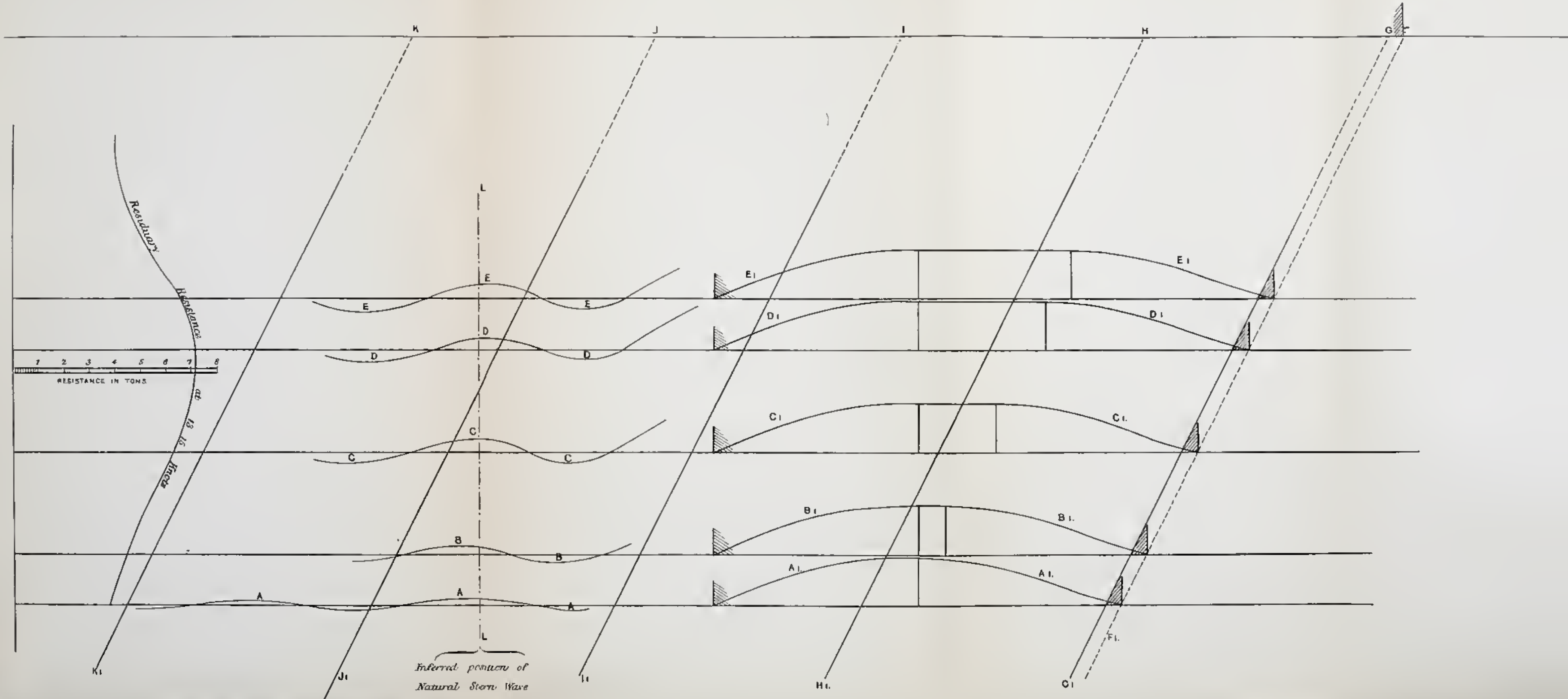


FIG II



AA, BB, CC, DD, & EE *Vertical Sections of Water Surface in the wake of the ships* AA, BB, CC, DD, & EE.

FG, FH, FI, FJ, & FK *Distances of successive wave crests from bow of longer ships, as appearing against parallel side*

HORIZONTAL SCALE 1 INCH = 40 FEET VERTICAL SCALE OF WAVE SECTIONS 1 INCH = 5 FEET

/ave Makin

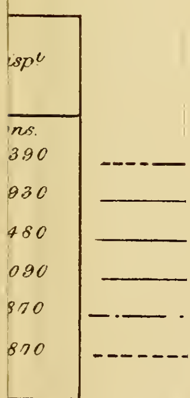
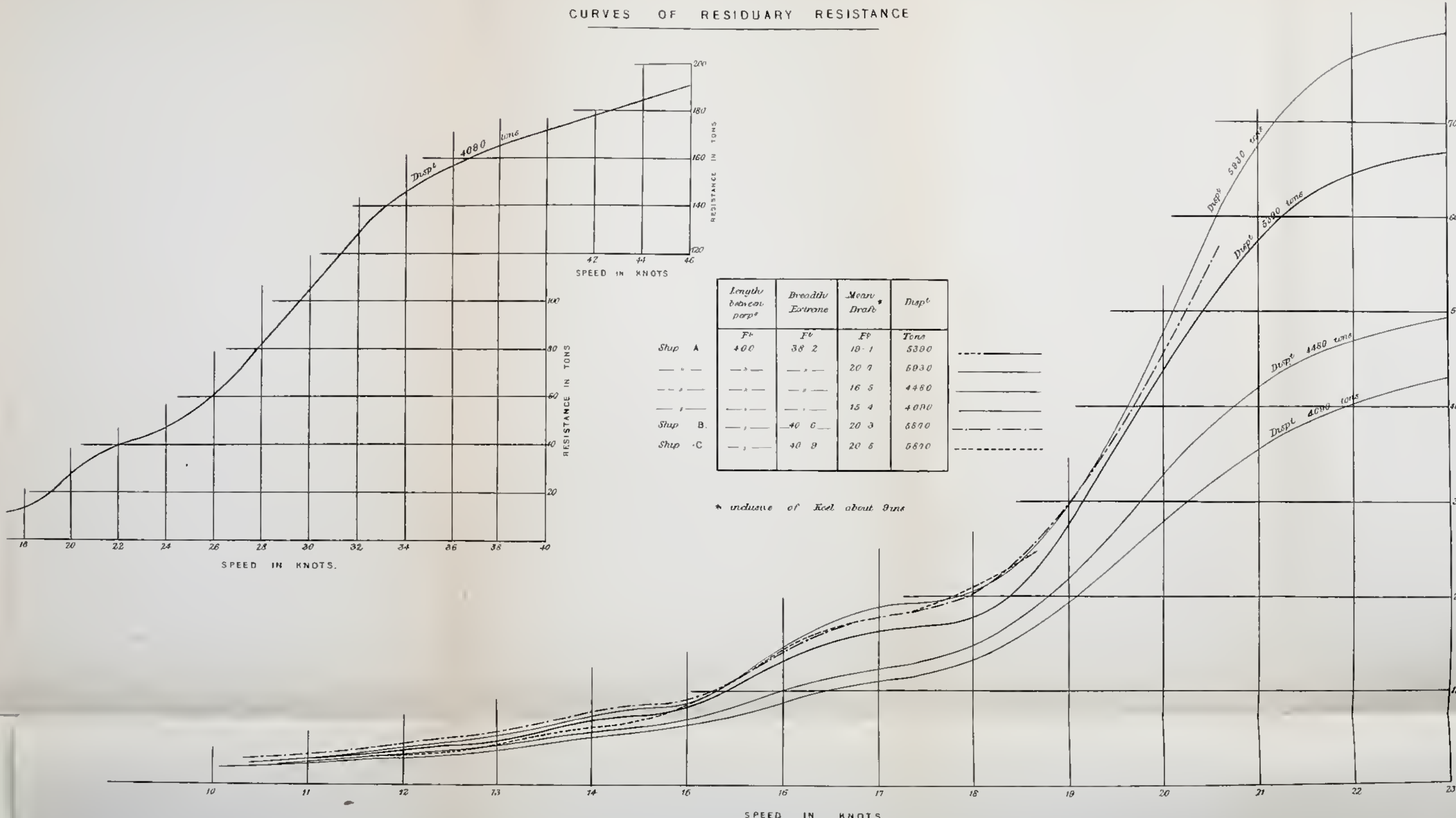


FIG 12.

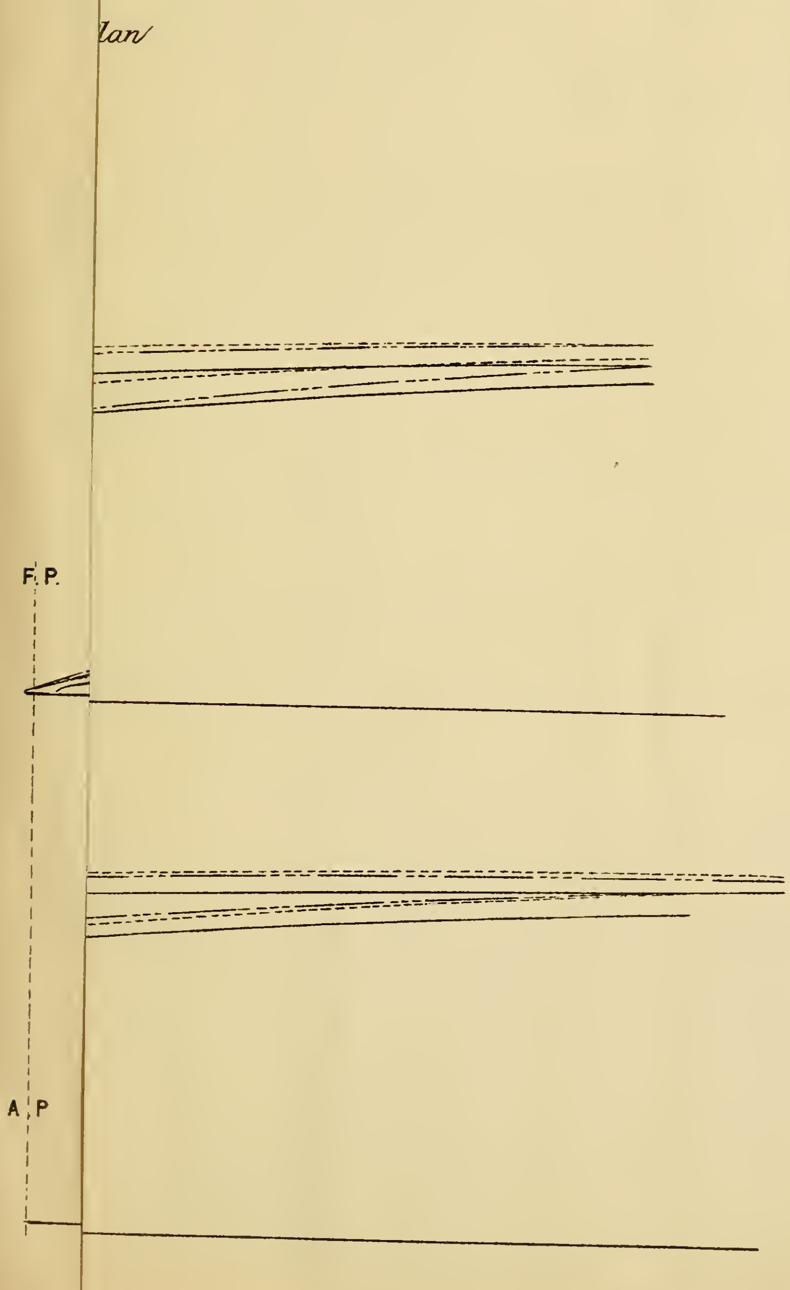
CURVES OF RESIDUARY RESISTANCE



	Length between perp's	Breadth Extreme	Mean Draft*	Dispt	
	Ft	Ft	Ft	Tons	
Ship A	400	38 2	18·1	5390	- - - - ———
— „ —	— „ —	— „ —	20 7	5930	———
— „ —	— „ —	— „ —	16 5	4480	———
— „ —	— „ —	— „ —	15 4	4000	———
Ship B.	— „ —	40 6	20 3	5870	— · — · —
Ship C	— „ —	40 9	20 6	5870	- - - - - -

* inclusive of Keel about 9ins

Resistance of Ships



To Illustrate Mr. R. E. Froude's Paper on the Leading Phenomena of the Wave Making Resistance of Ships

FIG 13

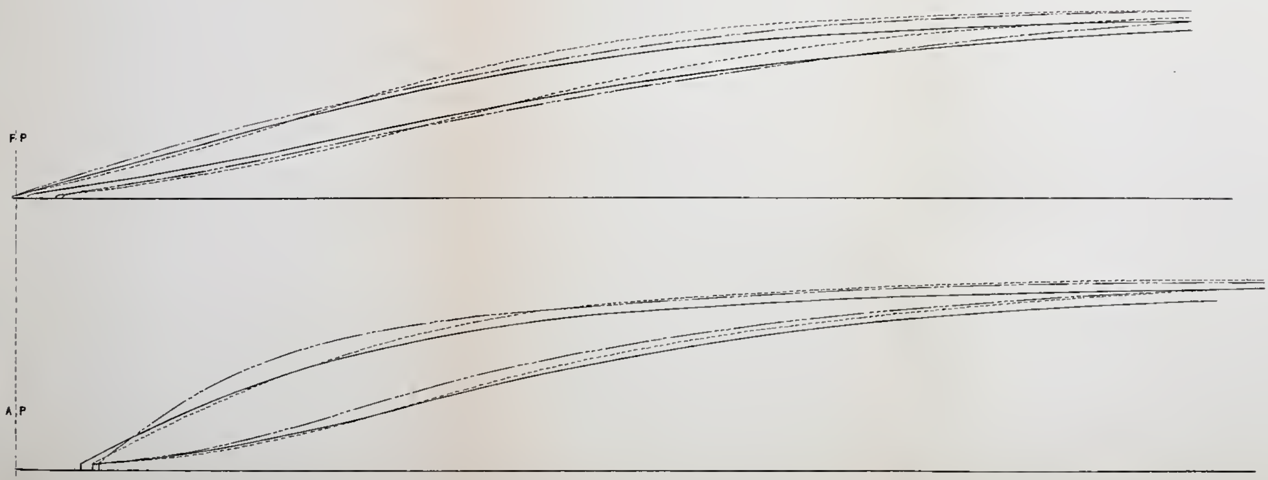
Load Water Line and Water line 10 ft below L W L. shown in Half Breadth Plan

SCALE 1/16 INCH = 1 FOOT.

Ship A ——————

B - - - - - -

C - - - - - -



6.

RATIO OF INDICATED TO EFFECTIVE HORSE-POWER AS ELUCIDATED BY MR. DENNY'S M. M. TRIALS AT VARIED SPEEDS.

By W. Froude, Esq., M.A., F.R.S.

[Read at the Seventeenth Session of the Institution of Naval Architects, 7th April, 1876.]

Mr. Denny has taken the bold but well-considered step of discarding the conventional type of measured mile trials which, as regards the speeds tried, have long been limited to full speed and half boiler power. Mr. Denny now tries each of his ships at four or even at five speeds; and the result is that he obtains fair data for a complete curve of indicated horse-power from the lowest to the highest speeds; whereas with trials on the ordinary system we obtain merely two spots in the curve, and these at comparatively high speeds, the intermediate or lower portion of the curve being left uninvestigated.

No doubt the limited view of the proper range of the inquiry to which the trials are intended to supply an answer arose from the belief that resistance must be as the square of the speed, and horse-power as its cube; and this belief, incorporated into one or other of the well-known "constants," has survived more or less persistently in spite of attacks and misgivings, and has constituted a self-supported obstruction to new ideas. It is also true, however, that M. M. trials, even as at present limited, are costly experiments, and notions of economy have assisted to damp the ardor of those who have been on other grounds willing to become innovators. But no expenditure ostensibly encountered in the search for truth is really so uneconomical as that which, while it seems to furnish information, helps to support error, and in fact "darkeneth counsel by words without knowledge;" and it is to Mr. Denny's honor that, finding the so-called constants were invariably variable and inconsistent, he determined of himself to strike out a new line and find out by trial what is fact, instead of contenting himself with assuming what ought to be the relation between indicated horse-power and speed.

A very interesting paper which he read in the mechanical section of the British Association at Bristol, illustrated by instructive diagrams which gave the results of trials conducted on his system, at once showed how fruitful a field of investigation he had opened up. And as he was on the point of trying a new ship from which he expected good results, he kindly promised to furnish me with her lines and with the report of the M. M. trials; and with the sanction of the director of naval construction, it was arranged that I should test the performance of a model of the ship with the admiralty apparatus, so as at once to see what was the relation between the ship's net resistance at all speeds and the power expended in overcoming it. The results of the investigation were unusually interesting and instructive.

On the one hand Mr. Denny's horse-power results when closely scrutinized were found at once to supply most important information on the subject of engine friction, and on the other they have helped to corroborate and further elucidate certain general conclusions on the subject of the expenditure of power in propulsion, which other less crucial tests had enabled me to arrive at approximately. The method of analysis to which I subjected the results is one which I have long adopted with advantage.

I have always felt that the system of reducing the results of steam trials to indicated horse-power, though no doubt furnishing a true expression in a commercial sense of the relative merits of the ship under trial, tended nevertheless to cloud the real significance of the record, viewed as suggestive of those specialities of form or condition which have really governed the ship's performance; not only because indicated horse-power includes in one large term the merits of the ship, the engine, and the propeller, but because the term into which it groups these items is complicated by the introduction of the speed factor, instead of representing them under their more elementary form of force simply. With this view, ever since I have entered into such investigations, I have invariably converted the horse-power term to a force term by simply dividing it by a speed factor, and, as shaping the reduction into its most natural and opposite form, I have adopted as the divisor the speed of the propeller, expressed, not by its revolutions nakedly, but by its revolutions × its pitch, that is to say, the virtual travel of the force delivered by the propeller. The result thus obtained from the indicated horse-power I have termed "indicated thrust;" it is in fact the thrust which the propeller would be exerting if the force of the steam were employed wholly in creating thrust, instead of partly in overcoming friction, driving the air-pump, and overcoming other collateral resistances. Indicated thrust is simply a constant multiple of the mean steam pressure on the piston; and if this were given in the records of the trials, indicated thrust is—

$$\frac{\text{mean piston pressure} \times \text{total piston travel per revolution}}{\text{pitch of propeller}};$$

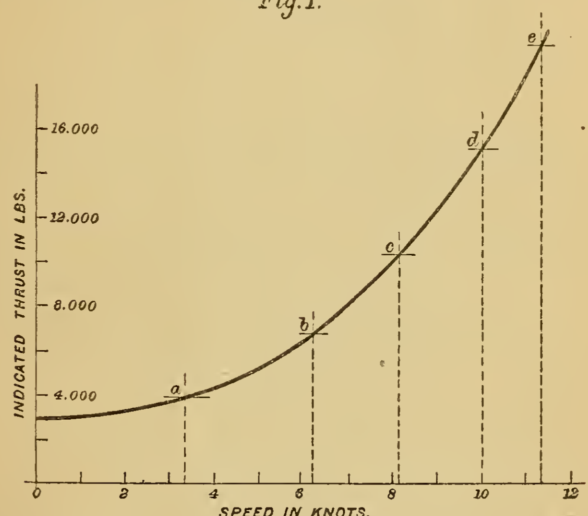
when however (as is commonly the case) the indicated horse-power alone is given, then the expressions for indicated thrust is $\dfrac{33{,}000 \times \text{I.H.P.}}{\text{pitch} \times \text{revolutions}}$.

When decomposed into its constituent parts indicated thrust is resolved into several elements, which must be enumerated and kept in view.

These elements are: (1) The useful thrust or ship's true resistance; (2) the augment of resistance, which, as I have pointed out in many previous papers, is due to the diminution which the action of the propeller creates in the pressure of the water against the stern end of the ship; (3) the equivalent of the friction of the screw-blades in their edgeway motion through the water; (4) the equivalent of the friction due to the dead weight of the working parts, piston packings, and the like, which constitute the initial or low-speed friction of the engine; (5) the equivalent of friction of the engines due to the working load; (6) the equivalent of air-pump and feed-pump duty.

It is probable that 2, 3, and 4 of the above list are all very nearly proportional to the useful thrust; 6 is probably nearly proportional to the square of the number of revolutions, and thus at least at the lower speeds approximately to the useful thrust; 5 probably remains constant at all speeds, and for convenience it may be regarded as constant,

Fig. 1.

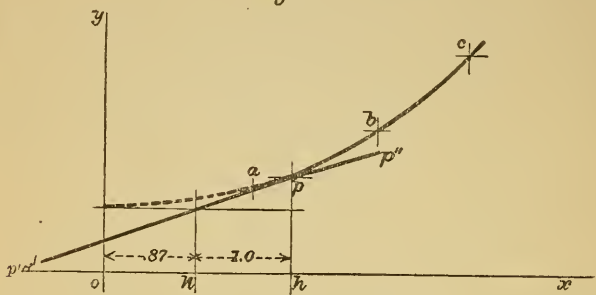


though perhaps in strict truth it should be termed "initial friction." If then we could separate the quasi-constant friction from the indicated thrust throughout, the remainder would be approximately proportional to the ship's true resistance. In point of fact, the means of performing this separation have been furnished by the conversion of Mr. Denny's indicated horse-power record into a curve of indicated thrust. The determinations of the initial friction of the *Merkara*, the *Taupo*, and *Hawea*, and of the *Greyhound* appear in Figs. 1, 2, and 3. But the circumstance by which the separability of the initial friction from the other forces makes itself apparent, and the method by which the separation may be effected, will be more readily understood by referring to the accompanying hypothetical sketches rather than to the finished diagrams.

Assume that the ordinates at *a*, *b*, *c*, *d*, and *e*, Fig. 1, represent to scale the several indicated thrusts referred to the appropriate speeds;

then a fair curve drawn through these points constitutes what I have called the "thrust curve." Now on drawing the curve with the data supplied by Mr. Denny's trials, it becomes at once manifest in every case that at its low-speed end the curve refuses to descend to the thrust zero, but tends towards a point representing a considerable amount of thrust, and it is impossible to doubt that this apparent thrust at the zero of speed, when there can be no real thrust, is the equivalent of

Fig. 2.



what I have termed initial friction; so that if we could determine correctly the point at which the curve, if prolonged to the speed zero, would intersect the axis *o y*, Fig. 2, and if we were to draw a line through the intersection parallel to the base, the height which would be thus cut off from the thrust ordinates would represent the deduction to be made from them in respect of constant or initial friction, and the remainders of the ordinates between this new base and the curve would, as has been explained, be approximately proportional to the ship's true resistance.

Now, the data do supply us with the means of fixing this intersection with considerable exactness in the following manner: The curve, as fixed by the data, terminates at some moderate speed, say 3 or 4, or 5 knots. It is well known now, that with tolerably well-shaped ships of such dimensions as those we are dealing with, the resistance due to such moderate speeds as these consists almost solely of surface-friction, which—as our experiments have shown varies nearly as the power 1.87 of the speed—with perhaps a very small residue or excess of resistance, apparently proportional to the square of the speed; and as this residue is very small indeed, we may without serious error assume that the whole resistance below 3 or 4 knots is as the power 1.87 of the speed. Hence on this assumption the lower end of the thrust curve when divested of the constant friction equivalent should be a parabola in which the ordinate is as the power 1.87 of the abscissa; and since, as we have seen, the entire thrust exclusive of the initial friction is proportionate, at least at the slow speeds, to the true resistance curve, the problem to be solved is the very simple geometrical one of so drawing a parabola of this order in connection with the axis of co-ordinates of the diagram, that it shall meet or join the existing thrust curve with an identical tangential direction. The construction by which this is effected is extremely simple; at the point p, Fig. 2, near the lower end of the thrust curve draw the tangent $p' p''$; draw the vertical at h' so as to cut the space $o\ h$ into segments having the ratio indicated by the figured quantities, thus making $o\ h = 1.87\ o\ h'$; draw a line parallel to $o\ x$ through the point where this vertical cuts the tangent; the point where this line cuts the thrust axis is the vertex of the required curve.

The parabolic completions thus applied to Mr. Denny's thrust curves are found not only to meet them at a common tangent, but to "osculate" them for a considerable distance, which confirms the belief that the operation is not merely a geometrical one, but expresses an action which is dynamically real.

Fig. 3 (Plate I) shows the process as completed from the records of the trials of Mr. Denny's ship, the *Merkara*. In this, as in the other similar curves, the black crosses show the points deduced from the steam pressures recorded in the individual trials. The curves of indicated horse-power and the curves of slip are also given.

A curious confirmation of the soundness of the method adopted and of the exactness with which it determines the initial friction is supplied by the diagram, Fig. 4 (Plate I), which represents the thrusts of the *Taupo* and the *Hawea*, which as sister ships ought to have experienced the same resistances throughout. The curves of indicated horse-power or these ships, however, exhibit a disagreement, and this of a kind which certainly does not at once suggest its true origin; but the corresponding indicated thrust curves, when completed in the manner described, show at once that the disagreement lies in a difference of indicated thrust which is throughout practically constant, and probably expresses in a constant difference between the initial frictions originates of the two ships' engines. The slight exception to the constancy of the difference which appears at one point of the comparison is manifestly due to an abnormal feature in the *Hawea's* curve, and this is such as to suggest that there has been some small error in the determination of the speed at the point which occasions it, for at this point there is a corresponding irregularity in the slip curve, and one and the same correction would obliterate both irregularities.

Moreover, through Mr. Denny I have received from his friend, Mr. Inglis, the reports of the trials of two ships built by the latter—the *Arbutus* and the *Pachumba*, Figs. 5 and 6 (Plate II); and these also, when similarly analyzed, give a precisely analogous result. On comparing these five curves thus analyzed and several others in which the analysis was not quite so simple, but was, I think, equally conclusive, it appears that the constant friction is equivalent to from one-eighth to one-sixth of the gross load on the engine when working at its maximum speed and power. And it is not irrational to accept this relation provisionally as the basis of an empirical formula, since the constant friction depends to a large extent on the diameter and weight of the working parts of the engine, and these must be approximately proportionate to the intended maximum strain, subject, of course, to some allowance for the variation which exists in the types of engine in use. I must admit that the proportion appears to me to be unexpectedly large, but the process by which it is determined is, I think, so certain and definite that I can not doubt the general soundness of the conclusion deduced by it; and that conclusion seems to me to be one of very high importance and significance; namely, that a screw engine when working at whatever speed, even its most moderate and economical speed, must be understood to be throwing away in the one element of this friction alone, not indeed one-seventh of its maximum power, for the engine may be now working at reduced speed, but a power due to one-seventh of its maximum load. Thus in the case of the *Merkara*, when the ship is steaming at 5 knots in a smooth sea one-half of her whole expenditure of power is due to this circumstance. The question of the apportionment of this large amount of inevitable friction between the several working parts of the engine and of the proportionate degree in which it attaches to different types of engine, as well as of the extent to which the evil is remediable, are inquiries of great importance, but they are more or less out of my reach, and are at all events beyond my present purpose, which is satisfied by the proof—an irresistible proof, as it appears to me—that the evil does exist about to the degree named.

But the discovery of the actual amount of power thus expended has been of great assistance to me in the attempt to account for the fact, of which accumulated proof exists, that the total power employed in a screw ship's propulsion greatly exceeds that required by the net resistance; and I venture to call the attention of the meeting to my investigation of this subject, as far as I have yet carried it.

The greatness of the excess has become manifest wherever it has been possible to compare a ship's actual resistance at a given speed with the indicated horse-power required to drive her at that speed, and abundant data for the comparison have been supplied in the first place by the dynamometric trials of H. M. S. *Greyhound*, and in the next by the experiments on the resistances of ships of various forms which I am carrying out for the Admiralty by careful dynamometric trials of their models; for among the latter it naturally happens that many forms which have had their net resistance thus determined have also been tried on the measured mile, and to these must be added Mr. Denny's ship, the *Merkara*. The result of the comparison shows that as a rule only from 37 to 40 per cent. of the whole power delivered is usefully employed,* and it will be seen that using the constant friction of the engines as an index of the scale of the friction generally, the 60 per cent. or more of loss can fairly be accounted for.

---

* In Mr. Denny's ship the usefully employed power is as much as 42 per cent. This, however, is at the light or trial draught.

In the earlier part of this paper I enumerated the principal elements which constitute the gross load of the engines. I repeat the list here: (1) Ship's net resistance. The power due to this I shall designate effective horse-power or E. H. P. (2) Augment of net resistance due to negative pressure created about the ship's stern by the action of the screw. (3) Water friction of screw. (4) Constant friction, or friction of engine as without external load. (5) Friction due to external load. (6) Air-pump and feed pump resistance.

The six elements are force factors, and when multiplied by

$$\frac{\text{speed of ship in feet per minute}}{33{,}000}$$

constitute the ship's horse-power as fundamentally due to her progress, and I shall designate this S.H.P. in making up the account, which will consist of the several elements all ultimately reduced into terms of E. H.P., which is in a sense the origin of them all.* The horse-power due to slip has to be added subsequently.

I proceed to quantify each element in detail, making up the account in the first instance for the highest speed as a point of departure.

(1) This is to represent the ship's true resistance whatever it be.

(2) The augmentation of the ship's resistance by the induced negative pressure under the stern, consequent on the thrust of the screw, is a circumstance on which I have often laid great stress at the meetings of this institution. There is abundant proof of its existence and approximately of its magnitude in the records of the trials of *Rattler* and *Alecto*, *Niger*, and *Basilisk.* Here I will only refer to the more crucial proof of its existence and of its magnitude which has been obtained by our experiments with models. After a model's net resistance at all speeds has been determined in the usual manner, the resistance is again determined under the conditions imposed by screw thrust. There is brought up behind the model, and quite independent of it, a screw shaft, carrying a screw of the required pitch and suitably speeded; the shaft being bracketed out forward from and beneath a horizontal frame, which possesses a delicate dynamometrically governed horizontal fore and aft mobility, and is suspended below a truck, which, at a definite distance astern, follows the dynamometric truck, by which the model's resistance is determined. The screw, placed exactly where it would be if it were driving the model, rotates with a speed sufficient to drive it,

* To defer thus the introduction of this form is equivalent to substituting the speed of the ship for the speed of the screw in all the power terms—in the E.H.P. as well as the rest. This arrangement is preferred on the ground that it appears to bring out with special distinctness the circumstance that all the elements enumerated, except slip, are alike virtual additions to the ship's resistance, and would equally exist if there were no slip, while the slip, taken separately and subsequently, represents the additional power expended on all the force elements alike, in consequence of the partial yielding of the point of reaction from which the propulsive force is taken. In virtue of the slip there are so many more revolutions per minute performed by the engine, with its total load—its superfluous load as well as its useful load.

but without touching it or effecting it except through the hydro-dynamic action which it is sought to measure. The force of rotation employed in driving the screw and the drag or thrust it exerts on the frame which carries it are both automatically recorded, and the speeding is varied until a speed is found at which the total thrust equals the model's total resistance; that is to say, its net resistance + the augment in question. These experiments show that with ships of ordinary form, the augment is from 40 to 50 per cent. of the ship's net resistance, and in making up the account I shall rate horse-power due to (2) as = 0.4 E.H.P.

(3) *Water friction of screw.*—The *Greyhound* experiments showed that the additional resistance, caused by the screw when it was allowed to rotate freely as the ship went ahead, considerably exceeded 0.1 of the ship's natural resistance. Now the speed with which the screw revolved was less than that due to the speed of the ship, and to have driven it at a higher speed would certainly have required more force, or in other words would have been a greater drag on the ship; it can not, therefore, be unfair to rate this as 0.1 of the natural resistance. Horse-power due to (3) is therefore = 0.1 E.H.P.

(4) *Constant friction due to dead weight and tightness of the moving parts.*—It has already been shown that this is at all speeds equal to about one-seventh of the total load on the engines when working with the maximum intended speed and pressure; and since the account is in the first instance taken, as it applies to the highest speed, I take the horse-power due to (4) as = 0.143 total S.H.P.

(5) *Friction due to working load of engine.*—This, at the maximum speed, can hardly be taken as being less than the dead load friction, when it is borne in mind that the forces to which it is due exceed by many times the dead weights of the moving parts, to which principally the dead load friction is due, I therefore rate it at the same amount, so that S.H.P. due to (5) is = 0.143 total S.H.P.

(6) *Air-pump resistance.*—According to Tredgold the load on the engine due to the air pump is between one-tenth and one-twentieth of the whole load on the engine. I shall set it down as 0.075. Thus the S.H.P. due to (6) is = 0.075 S.H.P.

The horse-power due to the several elements, worked out on the foregoing basis and combined, may be tabulated as follows:

Horse-power due to	No. 1 =	E.H.P.	
Ditto	No. 2 =	.4 E.H.P.	
Ditto	No. 3 =	.1 E.H.P.	
Ditto	No. 4 =		.143 S.H.P.
Ditto	No. 5 =		.143 S.H.P.
Ditto	No. 6 =		.075 S.H.P.

Or in combination— S.H.P. = 1.5 E.H.P. + .361 S.H.P.

So that— .639 S.H.P. = 1.5 E.H.P.

Or— $\text{S.H.P.} = \frac{1.5}{.639}\ \text{E.H.P.} = 2.347\ \text{E.H.P.}$

To this must be added—Slip = .1 S.H.P., making I.H.P. = 1.1 S.H.P.

Thus— I.H.P. = 2.582 E.H.P.

$$= \frac{100}{38.7} \text{ E.H.P.}$$

Or— E.H.P. = .387 I.H.P.

And this conclusion agrees very fairly with what, as I have already pointed out, more general experience has led me to adopt as an average expression of the relation between indicative and effective horse-power, namely, that at high speed the former is about 2.7 times the latter, or the latter $37\frac{1}{2}$ per cent. of the former.

To convert the formula from one adapted to high speed only to one adapted to all speeds it is necessary to keep the term involving constant friction separate from the rest, for it represents simply the effect of a constant resistance operating with the existing speed of the engine. In shaping the formula I shall adhere to the co-efficient 2.7, derived from rather broad experience, in preference to the co-efficient 2.582 just now built up on somewhat hypothetical data, assuming however, that the constant friction is equal throughout to one-seventh of the maximum load. Of the 2.7 E.H.P. which make up the I.H.P. at the maximum speed V, one seventh part or .385 is the part due to constant friction, leaving 2.315 as due to the other sources of expenditure of power. And to express the I.H.P. due to constant friction at any other speed v, we must alter the co-efficient in the direct ratio of the speed.

So that the term becomes $\frac{v}{V} \times .385 \times$ E.H.P. at designed maximum speed. Thus the formula for I.H.P. at any speed v is as follows:—

$$\text{I.H.P.} = 2.315 \text{ E.H.P.} + .385 \frac{v}{V} \times (\text{E.H.P. due to V});$$

Or if we finally sever the useful from the collateral expenditure of power, it stands thus—

$$\text{I.H.P.} = \text{E.H.P.} + 1.315 \text{ E.H.P.} + .385 \frac{v}{V} \times (\text{E.H.P. due to V}).$$

The several elements thus calculated are shown in combination in Fig. 7 (Plate III) approximately to fit the case of the *Merkara*, but the figure *mutatis mutandis* will represent pretty nearly their relative value in the case of other ships. The results are given not as expressing a complete solution, but as a well-considered step towards it.

To illustrate M^r. W.

A .

B .

C .

D .

To illustrate Mr. W. Froude's Paper on the Ratio of Indicated to Effective Horse Power.

FIG. 3

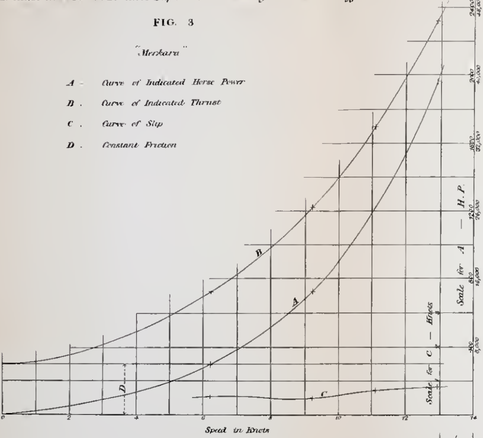
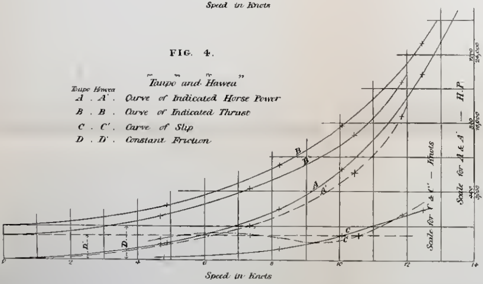


FIG. 4.



To illustrate Mr. W. Fro

To illustrate Mr. W. Froude's Paper on the Ratio of Indicated to Effective Horse Power.

FIG. 5.

"Arbutus"

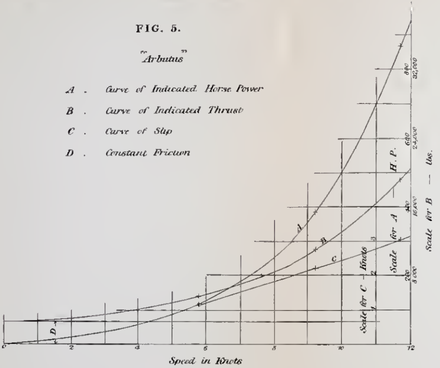
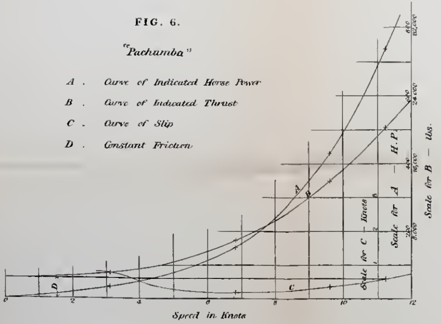


FIG. 6.

"Pachumba"



To illustrate Mr. W. F

To illustrate Mr. W. Froude's Paper on the Ratio of Indicated to Effective Horse Power.